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Auditor Specialization, Auditor Dominance, and Audit Fees: The Role of Investment Opportunities

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ABSTRACT: A report issued by the U.S. General Accounting Office (GAO) in 2003 identified auditors' industry expertise as a critical factor for firms choosing an auditor, and highlighted the extreme levels of auditor concentration in some industries. We posit that the investment opportunity set (IOS) plays a fundamental role in determining whether an industry is an attractive target for auditor specialization. When industry-specific IOS is high, specialist auditors make costly investments in industry-specific knowledge, allowing them to offer a differentiated product and to create entry barriers for other audit firms. When the IOS of firms within an industry is relatively homogeneous, auditors can transfer such knowledge across clients in the industry more easily, resulting in cost savings and scale economies. However, greater homogeneity of IOS in an industry can also increase a client's aversion to sharing an auditor with its competitors because of concerns about transfers of proprietary information, suggesting that industries with relatively homogeneous IOS are less likely to be dominated by a single auditor. We show that auditor concentration in an industry relates positively to both the level and homogeneity of IOS in the industry, while auditor dominance relates negatively to industry IOS homogeneity. Further, we find that audit fees are positively associated with both levels and homogeneity of industry IOS.

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I. INTRODUCTION

In July 2003, the U.S. General Accounting Office (GAO) published the results of its Mandated Study on Consolidation and Competition among public accounting firms.[1] While the GAO found no empirical evidence that consolidation had impaired competition in the market for audit services, it did suggest that future client choices could be limited further and that changes in the profession may have implications for competition and auditor choice, "especially in certain industries" (GAO 2003, preface). Specifically, the GAO identified auditor industry specialization as a key feature that is likely to affect the choice of an auditor in the future. The GAO recognized that there is a demand for auditors who are industry specialists and noted that the demand for specialists had led to extremely high two-audit-firm concentration ratios in some industries.[2] The study concluded that one of the factors driving consolidation of the large accounting firms was the need to build industry-specific expertise, and that industry specialization is an area that "may warrant ongoing and additional analysis" (GAO 2003, 53).

Academic researchers have had only limited success in identifying factors that motivate auditors to specialize, and Craswell et al. (1995, 320) conclude that "we do not know why specializations occur in some industries but not others." In this study, we argue that the industry investment opportunity set (IOS) is a fundamental driver of differences in industry specialization by auditors. The IOS is composed of "prospective investment opportunities and associated payoff distributions" (Smith and Watts 1992), and firm value may be viewed as comprising growth options and assets-in-place, where growth options increase a firm's IOS (Myers 1977; Skinner 1993). Growth options are difficult to observe and their value depends on managers' discretionary choices. As such, clients with higher levels of growth options are likely to have greater information asymmetry, which increases the importance of monitoring by auditors and directors (e.g., Anderson et al. 1993). While firms have an individual component to their IOS, industry IOS reflects the common nature of investment opportunities for firms in that industry. Smith and Watts (1992) assert that there is a significant industry component to individual firms' IOS. Consistent with this assertion, we describe an industry IOS using measures of central tendency (including homogeneity) of the IOS of the firms within that industry.

We suggest that auditors benefit from specialized knowledge in auditing firms with high growth options. Because it is costly to acquire industry-specific knowledge about the IOS, an auditor who makes this investment can become a specialist and create barriers to entry by being a low-cost producer or by offering differentiated products (i.e., higher quality audits). Thus, we expect industry specialization to increase with the level of industry IOS. Also, we expect that the variability of IOS across firms in an industry affects the attractiveness of that industry for specialization. When firms in an industry are more homogeneous in terms of their IOS, it is easier for an auditor to transfer industry-specific knowledge from one client to another. This ease of transfer suggests a positive relation between the homogeneity of IOS and the extent of specialization.

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[1] The GAO study was authorized by section 701 of the Sarbanes-Oxley Act of 2002. The objectives of the study were to examine (1) factors leading to mergers of the large public accounting firms in the 1980s and 1990s, (2) the effect of consolidation of the accounting firms on competition, (3) the impact of consolidation on auditor independence and the cost and quality of audits, (4) the impact of consolidation on capital markets, and (5) the barriers that smaller audit firms encounter in competing with the largest accounting firms.

[2] The GAO reported that 80 percent of the companies responding to its survey said that industry specialization or expertise would be of either "great" or "very great" importance if they had to choose a new auditor. Specialization ranked behind only service quality and auditor reputation as factors influencing auditor choice. Comparing 1997 to 2002, the report finds more industries in 2002 where two firms accounted for over 70 percent of the total assets audited. For example, they find that, by 2002, Ernst & Young and PricewaterhouseCoopers audited 90.7 percent of assets of firms in the metal mining industry, while Deloitte & Touche and Ernst & Young audited 80.1 percent in general building contracting (see GAO 2003, 116).

We also expect that industry IOS will affect the ability of a single specialist auditor to dominate an industry. In particular, while greater homogeneity of industry IOS increases the incentives for auditor industry specialization, it will make it harder for a single specialist to dominate an industry. Following Kwon (1996), we recognize that clients in highly competitive industries could be averse to using the same auditors as their competitors for fear that proprietary information could be transferred, either directly or indirectly (e.g., through auditor benchmarking of other clients' financial statements against theirs). Firms will be more concerned about the transfer of proprietary information when they and their competitors face similar opportunities. Accordingly, we predict that the homogeneity of industry IOS is negatively related to auditor dominance in an industry.

If industry-specific knowledge about the IOS is an important factor in the auditor's production function, then industry IOS is also likely to affect the pricing of audits. We expect industry IOS to be related to fee premiums, where the premiums can be viewed either as compensation for the added costs and complexity of auditing clients with greater growth opportunities, or as payment for the imprimatur of a specialist auditor. The relation between IOS homogeneity and audit fees is less clear-cut. Greater homogeneity enables auditors to spread their industry-specific knowledge costs across more firms, leading to scale economies and potentially lower fees. However, if greater homogeneity reduces competition among auditors as firms avoid their competitors' auditors, then an opportunity arises for auditors to charge *higher* fees. Our expectation is that auditors will exploit this opportunity to charge higher fees as industry IOS homogeneity increases.

Consistent with the measures used by the GAO, as well as with much of the academic accounting literature to date (e.g., Hogan and Jeter 1999; Balsam et al. 2003; Mayhew and Wilkins 2003), we choose auditor concentration based on client size as our primary measure of industry specialization.[3] In our sensitivity analyses, we also consider two alternative measures of industry specialization: a composite measure reflecting the importance of a given industry in the auditor's portfolio and a residual measure aimed at removing the effects of industry structure, both adapted from Cairney and Young (2006).[4] Similar to Kwon (1996), we use two measures of auditor dominance based on the dispersion and average number of clients in an industry. Our specialization and dominance tests use Compustat data from 1986 to 2004. Because our audit fee tests require audit fee data, we use Audit Analytics data from 2001 to 2004 for these analyses.

We find higher levels of auditor specialization when industry IOS is higher and when IOS within the industry is more homogeneous, consistent with our argument that IOS is a fundamental determinant of auditor specialization. Further, auditor dominance is negatively associated with industry IOS homogeneity, also as predicted. Thus, while industry IOS affects the attractiveness of an industry for specialization by the auditor, increasing industry IOS homogeneity also makes it more difficult for a single specialist to dominate an entire industry. Additionally, we find that industry IOS levels and homogeneity are both positively related to audit fees. This supports the view that specialists charge higher audit fees to recoup their higher audit production costs or take advantage of their clients' aversion to sharing auditors with competitors, particularly when the potential for proprietary information transfer is great. Overall, our results provide evidence that the IOS affects both the structure of audit markets and the pricing of audits within industries.

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[3] As the GAO (2003) points out, an industry specialist could audit a large majority of firm assets in a particular industry even though it audits only a few firms. In contrast, another auditor might audit many smaller companies, but not have the "requisite excess staff capacity or technical expertise necessary to handle the larger clients in that industry," as implied by the term specialization. Industries conducive to specialization would tend to preclude other firms from easily entering the market and challenging specialist firms' market share" (GAO 2003, 27, note 18).

[4] Our results are generally robust to these specification changes, as discussed subsequently.

We contribute to the extant literature in several ways. First, we extend prior studies examining auditor specialization and dominance by linking the structure of audit markets to industry IOS.[5] Second, we contribute to the audit fee literature that examines pricing by industry specialists. Unlike prior studies that use auditors' market shares to identify specialists and then examine the relation between the market share and audit fees, we test directly whether, and how, audit fees are associated with industry IOS.

The remainder of this paper is organized as follows. Section II provides background information, and Section III develops our hypotheses. Sections IV–VI describe our method, data, and empirical results. Section VII contains sensitivity tests, and Section VIII summarizes and concludes.

II. BACKGROUND

Reasons for Industry Specialization

Prior academic research supports the GAO's view that auditors seek to specialize in certain industries. However, only a few studies attempt to explain cross-industry variations in specialization. Consequently, as Craswell et al. (1995) suggest, it is not clear why auditors specialize in some industries to a greater extent than in others. In an early series of studies, Eichenseher and Danos (1981) and Danos and Eichenseher (1982) argue that cost savings arising from economies of scale can explain industry specialization by auditors. They suggest that industry specialists spread the costs of knowledge acquisition and training across more clients, and that knowledge requirements are higher in regulated industries. Extending this line of research, Hogan and Jeter (1999) find that auditor specialization in regulated (nonregulated) industries is unchanged (increasing) over time, implying that industry-specific knowledge requirements apply to nonregulated industries as well as regulated industries, and that industry-related knowledge requirements have been increasing over time. Cairney and Young (2006) find that auditor specialization is higher in industries where firms are more homogeneous in terms of their operating expenses. While these studies provide useful insights into industry specialization, none focuses directly on the underlying innate firm/industry characteristics that drive the auditors' specialization choices.

Auditor Industry Dominance

The research on auditor dominance may be viewed as an extension of the specialization literature. Danos and Eichenseher (1982) suggest that high levels of client intra-industry competition cause clients to hire different auditors from their competitors to prevent intra-industry information transfers (also see Verrecchia 1983). Kwon (1996) develops this argument explicitly and finds a negative association between measures of auditor industry dominance and client industry concentration. Using the speed of the intra-industry adjustment of abnormal profits to proxy for intra-industry competition, Krishnan (2005) similarly finds a negative association between auditor dominance and intra-industry competition. We extend this line of research by investigating the link between the IOS and client intra-industry competition in the context of auditor domination.

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[5] Anderson et al. (1993) also link IOS to auditing, but they do not consider whether IOS affects specialization within audit markets.

Audit Pricing by Industry Specialists

Several studies examine audit pricing by industry specialists. For example, Craswell et al. (1995) investigate whether industry specialists earn fee premiums over and above brand name premiums. They recognize that industry specialization can lead to higher fees since specialists need to be compensated for their investment in industry-specific knowledge. However, they also note that industry specialization can reduce fees if specialists benefit from production economies to a greater extent than non-specialists. Using Australian data, Craswell et al. (1995) find evidence of a premium for specialist Big 8 auditors over non-specialist Big 8 auditors. However, Ferguson and Stokes (2002), also using Australian data, find limited evidence of specialist premiums when alternative definitions are used to identify industry specialists.

While Craswell et al. (1995) consider the net audit fee premium or discount, Mayhew and Wilkins (2003) attempt to separate the premium and discount components. They argue that an increasing market share in an industry gives rise to production economies that are likely to lower fees. They also argue that a market share that is significantly higher than competitors' shares provides evidence that an industry specialist auditor offers a differentiated product. Using IPO data, they find that audit fees decrease as the auditor's industry market share grows but *increase* if the auditor has a dominant share. Casterella et al. (2004) focus on bargaining power in explaining specialist audit fees, and find that small clients pay a specialist premium while large clients do not. They interpret this as evidence that large clients have sufficient bargaining power to negotiate any specialist premium away.

III. HYPOTHESES

Auditor Industry Specialization

As the GAO (2003) documents, the level of specialization varies widely across industries. This begs the question: What makes some industries more attractive targets for specialization than others? O'Keefe et al. (1994) suggest that providing audit services to a client requires investments in general knowledge, industry-specific knowledge, and client-specific knowledge. *General knowledge* (e.g., knowledge of GAAP or GAAS) is important for conducting an audit, but is not a dimension for industry specialization since all auditors need to make this investment. In contrast, *industry-specific knowledge* and expertise are transferable, but only across clients in a given industry. Differences in industry specialization by auditors arise from differential industry-specific knowledge and expertise. In contrast, *client-specific knowledge* is acquired as part of a particular audit engagement and cannot be transferred to other clients.

An investment in industry-specific knowledge can affect the audit in two non-mutually exclusive ways. It can reduce the cost of producing the audit relative to non-specialists (who also need to acquire this knowledge) since specialists can spread their costs over a wider number of clients. This may be referred to as the production economies argument (see Eichenseher and Danos 1981). Specialization can also improve the quality of the audit. Following DeAngelo (1981), investments in industry-specific knowledge create industry-specific quasi-rents, which in turn lead to incentives for the auditor to be less independent (i.e., "cheat") to retain those rents. However, the greater the auditor's share of the industry, the less incentive the auditor has to "cheat," because the specialist auditor stands to lose the quasi-rents from its *other* clients in the industry if caught cheating. In other words, the other clients serve as collateral against opportunistic behavior by the specialist, resulting in a higher audit quality.[6] Barriers to entry play a crucial role in preventing specialists from being imitated and, thus, losing their competitive advantage. Fewer clients to provide collateral, as well as to spread investment costs across, put the entry-seeking non-specialist at a disadvantage, both in terms of independence and production costs.

We expect that the industry's attractiveness for specialization will be directly related to the amount of industry-specific knowledge requirements needed to complete the audit, and that these requirements are likely to vary widely across industries (see, e.g., Craswell et al. 1995). Some industries are more generic than others in terms of accounting technologies and audit risk.[7] For example, auditing a firm that manufactures washing machines is likely to be closer to auditing a firm that manufactures farm irrigation pumps than to a firm developing solar cells based on nanoparticle technology. The greater the industry-specific knowledge, the stronger are the barriers to entry that the auditor can build. We posit that the IOS is a primary and fundamental source of variation in the level of industry-specific knowledge that is required of auditors in different industries.

Smith and Watts (1992), Gaver and Gaver (1993), and Christie (2002), among others, highlight the fundamental nature of the IOS.[8] For example, Christie (2002) contends that the firm's IOS will affect its assets, customers and revenue uncertainty, employees (e.g., skill levels), technological uncertainty, growth, litigation risk, regulation, tax structure, and the type of knowledge needed to operate the firm. In turn, these factors affect the firm's organizational design, which includes the firm's marketing policy, production technology, compensation policy, capital structure, dividend policy, hedging policy, and accounting systems. Empirically, Smith and Watts (1992) and Gaver and Gaver (1993) provide supporting evidence of these associations at the industry and firm levels, respectively.[9]

Since their IOS affects firms' business models and the way they operate, we expect that it will also affect firms' monitoring systems, including audits.[10] While we focus on the role of auditing in monitoring IOS, we acknowledge that it is not the only monitoring mechanism available for this purpose, nor is it necessarily the primary mechanism.[11] At an industry level, auditing will be more difficult when firms' average IOS is relatively high. Myers (1977) contends that a firm's value can be broken into real assets (assets-in-place) and real options (growth options). Assets-in-place are largely tangible assets, whereas growth options are generally intangible and can include "capacity expansion projects, new product introductions, acquisition of other firms, investments in brand names through advertising, and even maintenance and replacement of existing assets" (Gaver and Gaver 1993, 127). Because growth options depend on managers' future discretionary spending and are hard to observe, they are difficult to value and their presence can create incentives for managerial opportunism. For example, Skinner and Sloan (2002) find that managers of growth firms have strong incentives to meet earnings forecasts. Thus, information asymmetry is greater and monitoring is likely to be more difficult in high IOS firms (e.g., Smith and Watts 1992).

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[6] At an empirical level, there is ample evidence that specialists provide higher quality audits. Specialist auditors are found to have greater nonerror knowledge (Solomon et al. 1999), make better assessments of risk (Low 2004), and detect more conceptual errors (Owhoso et al. 2002). In addition, firms employing industry specialist auditors appear to have higher earnings quality (Balsam et al. 2003) and higher disclosure quality (Dunn and Mayhew 2004).

[7] Accounting technology refers to the firm's reporting system and the choice and application of accounting methods (Craswell et al. 1995).

[8] Other recent examples include Goyal et al. (2002), Christie et al. (2003), and Ho et al. (2004).

[9] Smith and Watts (1992) use industry-level data from 1965 to 1985 and find lower leverage, lower dividend yields, high executive compensation, and more stock-based compensation plans in industries with high IOS. Smith and Watts (1992) conclude that IOS is more important in explaining fundamental corporate policy choices than signaling or tax-based theories. Gaver and Gaver (1993) provide corroborating evidence using firm-level data.

[10] The starting point for any audit is for the auditor to have a thorough understanding of the client's business environment and business model (e.g., Bell et al. 1997; APB 2003).

[11] For example, Anderson et al. (1993) argue that firms with more growth options are likely to spend relatively more on directors than on auditing since directors are more actively involved with management in terms of supervising their actions, advice, and approving/vetoing investment decisions.

Investment opportunities can affect audit risk in various ways. Managerial opportunism associated with high IOS increases inherent risk. Also, control risk increases because growth options are harder to monitor. As a result, designing and maintaining effective internal controls becomes more difficult. Furthermore, detection risk increases because of the complexity and uncertainty that growth options add to the audit process. For example, high IOS firms tend to be characterized by rapid change, and designing appropriate audit procedures becomes more complicated in such a setting (e.g., analytical procedures such as traditional ratio analysis may be less relevant). To provide a high-quality audit in this setting of heightened risk, the auditor needs specialized knowledge to plan and conduct the audit effectively. This specialized knowledge could be acquired through hiring decisions, through consultation with experts in a given industry, or through experience, i.e., a Bayesian outcome-feedback process.[12] Industry-specialized knowledge gives the auditor a better understanding of the client's demand and cost functions (and earnings distribution), which are likely to be unstable in a high IOS environment,[13] potentially reducing audit-judgment errors.

To the extent that a firm's IOS is a function of the firm's line of business or industry (e.g., Smith and Watts 1992; Christie 2002), the auditor's knowledge and expertise in detecting IOS-related errors and irregularities in an industry can be applied to all its clients in the industry (see, e.g., Bedard and Biggs 1991; Johnson et al. 1991; Wright and Wright 1997; Solomon et al. 1999; Low 2004). However, by definition, industry-specific knowledge cannot be transferred beyond the industry; thus, an auditor who is a specialist in one high IOS industry will not necessarily be a specialist in another high IOS industry.

In summary, a client's demand for a specialist auditor is determined by complex accounting and auditing issues associated with the nature and levels of industry-specific IOS. The higher costs of obtaining industry-specific knowledge in high IOS industries create barriers to entry by other auditors, enabling the specialist to capture a larger market share, which can lead to lower audit production costs and higher quality audits. Consequently, we expect that industries with high IOS, on average, will attract specialists more frequently than industries with lower IOS. We therefore hypothesize:

> **H1:** Industry IOS levels are positively associated with the extent of auditor specialization in the industry.

While H1 focuses on the average level of IOS in an industry, we expect that *intra-industry variation* in IOS will also affect the attractiveness of an industry for specialization. Cairney and Young (2006) argue that homogeneous industries are better candidates for specialization because auditors can use the same audit approaches and procedures for all their clients in an industry. They show that industries with similar cost structures (proxied as the correlation of changes in industry firms' operating expenses) are more likely to have specialist auditors. Similarly, we expect that industries where firms have more homogeneous levels of IOS are better candidates for specialization because greater homogeneity suggests a larger market over which to spread production costs and to benefit from quality differentiation. Thus, we hypothesize:

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[12] For example, several studies (e.g., Morgan and Stocken 1998; King and Schwartz 1999; Solomon et al. 1999) show that the learning effect has a positive impact on audit quality.

[13] In addition to its effect on the production of the audit, the IOS can also affect the demand for the specialist's nonaudit services (e.g., Addams and Davis 1994; Addams and Allred 2002; Wooten 2003) and client satisfaction (Behn et al. 1997).

> **H2:** The degree of homogeneity in industry IOS is positively associated with the extent of auditor specialization in the industry.

We use the variability of firm-level IOS within an industry to measure the homogeneity of industry IOS.

Auditor Industry Dominance

Thus far, our arguments have focused on why auditors would be prepared to invest in specialization. However, following a line of argument developed in Danos and Eichenseher (1982), Kwon (1996) suggests that the demand for specialists can be limited in certain cases as clients avoid being audited by the same auditor as their nearest competitors due to concerns about the transfer of proprietary information. He expects this effect to be more pronounced when firms in an industry are highly competitive with each other, as proprietary information becomes more relevant and potentially more damaging.

While firms in all high IOS industries may be highly competitive and have significant proprietary concerns, transfers of proprietary information become most costly when there is a firm with very similar opportunities poised to take advantage of the information. As such, it is when client firms within an industry are highly competitive, *and* have very similar investment opportunities (i.e., relatively homogeneous industry IOS) that we expect them to be averse to having the same auditor as other firms in the industry.

In summary, greater intra-industry similarity suggests that firms will be competing head-to-head in the same markets, causing proprietary information to be more highly valued by competing firms. Since auditors represent a possible channel for transferring such knowledge, we suggest that client firms in relatively homogeneous industries will be more inclined to avoid their nearest competitor's auditor, thus reducing the likelihood of a dominant industry specialist. These knowledge transfers could be either direct, such as inadvertent comments or advice from the auditor, or indirect (such as benchmarking analyses), with the latter being potentially just as damaging. Consistent with these arguments, Krishnan (2005) finds a negative association between auditor dominance and intra-industry competition, but does not examine the relation between dominance and the IOS.

Since the IOS is an innate driver of a firm's operating, investing, and financing decisions, firms with similar IOS are likely to be highly competitive; thus, industry IOS homogeneity is likely to be associated positively with proprietary concerns. Applying Kwon's (1996) arguments, we predict that increasing IOS homogeneity will lead firms in some cases to choose auditors different from their competitors. Thus, for a given level of industry IOS, as industry IOS homogeneity increases, it is less likely that a single auditor will dominate the market (i.e., there will be multiple industry specialists).[14] Accordingly, we predict:

> **H3:** Controlling for the level of industry IOS, auditor dominance is negatively associated with the homogeneity of industry IOS.

Audit Fees

If industry IOS is a fundamental driver of industry specialization, as we expect, then it is likely to affect audit pricing also. However, as Craswell et al. (1995) point out, this effect is not straightforward. Specialists might command a fee premium if they succeed in providing a differentiated product (higher quality audit) that adds value for their clients. Alternatively, specialist auditors might pass their cost savings along to clients by charging lower fees if they benefit from scale economies. Mayhew and Wilkins (2003) attempt to infer from the auditor's market share whether the auditor is a product differentiator or a low-cost producer.

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[14] On the other hand, holding industry IOS homogeneity constant, we expect that the probability of auditor dominance increases as industry IOS increases. High IOS industries are not only attractive for auditor specialization (H1), but they also provide a possible opportunity for dominant auditors to earn monopoly rents, thus enticing auditors to strive for dominance.

We expect that audit fees are related to the levels and homogeneity of industry IOS, in part due to their relation to auditor specialization (H1 and H2) and auditor dominance (H3). Specifically, we suggest that if industry IOS levels affect auditors' production functions, they should also affect audit fees. That is, we expect that auditors in high IOS industries will charge higher fees to recover the high costs of investments in industry-specific knowledge. Following our arguments, auditors could increase fees to compensate for the difficulty of auditing clients with high IOS. Even with a deep understanding of the client's industry, auditing a firm with high growth options can require extra time and effort to assess the risks associated with the exercise and measurement of the growth options, to detect errors if they exist (e.g., Owhoso et al. 2002), to identify earnings management attempts (e.g., Balsam et al. 2003), and to assure that disclosures and valuations (and possibly write-downs) in the financial statements are adequate and fairly represented (e.g., Godfrey and Hamilton 2005). Further, if greater industry IOS allows specialists to better differentiate their product (e.g., offer a higher quality audit because of their more extensive industry knowledge), then fee premiums should be larger in high IOS industries. Finally, if greater industry IOS allows specialists to gain market share and market power, then we expect that the ability of specialists to charge monopoly rents will be enhanced in those industries. Accordingly, we hypothesize:

> **H4:** The level of IOS in an industry is positively associated with the level of audit fees of clients in the industry.

The auditor's ability to spread the costs of acquiring industry knowledge and expertise across its client base in an industry depends on the homogeneity of IOS in the industry (e.g., Eichenseher and Danos 1981; Danos and Eichenseher 1982). Client homogeneity makes it easier for auditors to spread these costs because industry-specific knowledge will relate to these clients approximately equally. Holding the level of industry-specific knowledge constant, the existence of homogeneous clients implies greater economies of scale. Production costs are reduced, and to the extent that these cost savings are shared with other clients, increasing homogeneity leads to potentially lower audit fees.

On the other hand, since we expect that greater homogeneity will increase the extent of specialization (see H2), it will also increase the likelihood that specialists offer a quality differentiated product that can be priced at a premium. Further, as argued in relation to auditor industry dominance (see H3), greater homogeneity of IOS within an industry could actually limit a client firm's auditor choices since a given client has closer competitors and more auditors that it wishes to avoid. This lack of choice could give the engaged auditor more pricing power and lead to *higher* audit fees. This is conceptually similar to the argument of Casterella et al. (2004), who examine the auditor's bargaining power.

On balance, we expect that auditors will not choose to pass on the (full) cost savings due to economies of scale unless there is sufficient market pressure to do so. Thus, we predict that the effect of product differentiation and bargaining power will dominate the effects of economies in highly homogeneous industries. This leads to the following hypothesis:

> **H5:** The degree of homogeneity of industry IOS in an industry is positively associated with the level of audit fees for clients in the industry.

Interactions between Industry IOS Levels and Homogeneity

We also consider the interplay between the level and homogeneity of industry IOS.[15] Our theory suggests that an industry's attractiveness for specialization and audit fees within the industry will be jointly affected by the levels and similarity of industry IOS. More specifically, we expect that:

1. Industries that have both high (low) levels and high (low) homogeneity of industry IOS will be the most (least) attractive for auditor specialists. This suggests that the interaction between the level and homogeneity of industry IOS will be positively related to industry specialization.
2. The effect of homogeneity of industry IOS on auditor dominance will be most pronounced when the level of industry IOS is high (i.e., when the cost of losing proprietary information is particularly high). Thus, we expect that the interaction between the level and homogeneity of industry IOS will be negatively related to auditor dominance.
3. The expected association between the level of industry IOS and audit fees should be stronger in industries with homogeneous IOS, because greater homogeneity enhances opportunities for product differentiation and provides specialists with greater bargaining power (due to clients' aversion to sharing auditors). As a result, the interaction between the level and homogeneity of industry IOS is expected to be positively related to the audit fees within the industry.

IV. INDUSTRY SPECIALIZATION—ANALYSIS AND RESULTS

Method

To examine H1 and H2, we measure industry specialization using a two-auditor concentration ratio, measured in terms of total client assets, as our dependent variable (*ACR2*). This contrasts with Hogan and Jeter (1999), who used a three-audit firm concentration ratio but whose data end in 1993 when there were still six large accounting firms. As our data extend through 2004, when there were only four large accounting firms, we believe that the portion of the market captured by the top two auditors (rather than three) captures a more appropriate sector for measuring the extent of specialization. We also include controls for the number of large audit firms (varying from eight to four) in each year in our sample period.

For our IOS measure, we use scores from a factor analysis developed by Baber et al. (1996). The factor analysis includes the following four variables: (1) prior investment intensity for years t–2 through t, (2) the geometric growth in the market value of assets from years t–2 through t, (3) the ratio of the market value to book value of assets at the end of year t, and (4) the ratio of research and development expenditure to book value of assets at the end of year t. We rely on these scores to compute both an industry-specific IOS measure (*IIOS*) and our IOS homogeneity measure (*HIOS*).

Industry IOS is measured as the median of the firm-level IOS factor scores in a given industry. Higher *IIOS* measures reflect higher underlying IOS. To measure *HIOS*, we use the within-industry standard deviation of the firm-level IOS factor scores. When IOS is extremely variable, this suggests that a large component of IOS is firm-specific. To facilitate interpretation, we multiply the within-industry standard deviation by −1. Thus, we interpret high values of *HIOS* (i.e., less variation) as greater homogeneity of IOS in an industry and low values of *HIOS* (i.e., more variation) as a proxy for more heterogeneous IOS.

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[15] We thank an anonymous referee for prompting us to consider these interaction effects.

We test H1 and H2 using the following model adapted from Hogan and Jeter (1999):

$$ACR2_{kt} = \beta_0 + \beta_1 IIOS_{kt} + \beta_2 HIOS_{kt} + \beta_3 IIOS_{kt} * HIOS_{kt} + \beta_4 TIME_{kt} + \beta_5 REG_{kt} + \beta_6 CCR4_{kt} + \beta_7 LITRISK_{kt} + \beta_8 MEANSIZE_{kt} + \beta_9 BIG8_{kt} + \beta_{10} BIG6_{kt} + \beta_{11} BIG5_{kt} \quad (1)$$

$ACR2_{kt}$ is the market share of the two largest auditors (measured in terms of total client assets) in industry *k*. *IIOS* is the median value of IOS in industry *k* where IOS is measured using the factor score based on Baber et al. (1996). *HIOS* is (−1) times the standard deviation of IOS in industry *k*, and *IIOS***HIOS* is the interaction between *IIOS* and *HIOS*. *TIME* is a linear time effect variable (*TIME* = 1,2,...,19), and *REG* is equal to 1 if industry *k* is regulated, and 0 otherwise.[16] *CCR4* is the four-firm industry concentration ratio for industry *k* in year *t*, and *LITRISK* equals 1 if industry *k* is a high litigation-risk industry, and 0 otherwise.[17] *MEANSIZE* is the average size, measured as the square root of total assets (measured in millions) of all firms in industry *k* in year *t*. *BIG8*, *BIG6*, and *BIG5* are indicator variables where *BIG8* equals 1 for 1986–1989, *BIG6* equals 1 for 1990–1996, and *BIG5* equals 1 for 1997–2001; otherwise, the measures are 0.

We select our control variables based on prior research as well as our own intuition regarding those factors most likely to be associated with auditor concentration levels. The *REG* variable controls for the higher auditor concentration in regulated industries, consistent with Danos and Eichenseher's (1982) argument that specialist expertise is more valuable in industries that are subject to stringent reporting requirements and constraints. *TIME* is included to control for the trends in specialization documented by Hogan and Jeter (1999). The industry concentration ratio, *CCR4*, controls for the impact of industry structure on auditor concentration, if any. We include the likelihood of litigation, *LITRISK*, in a given industry and the average size of firms in the industry, *MEANSIZE*, to control for factors other than IOS that might influence the auditors' choice of targets for specialization, with the former making a particular industry relatively less attractive and the latter making it more attractive to auditors, *ceteris paribus*. Based on Hogan and Jeter (1999), we expect positive coefficients for all these control variables except *LITRISK*, which we expect to be negative. Finally, *BIG8*, *BIG6*, and *BIG5* further control for changes in specialization over our sample period, as the number of large auditors went from eight to four.[18]

Based on H1 and H2, we expect positive coefficients for both *IIOS* and *HIOS*. We also expect that the coefficient for the interaction between *IIOS* and *HIOS* will be positive.

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[16] Following Hogan and Jeter (1999), we classify firms in the following two-digit SIC codes as regulated: 10 (metal mining), 12 (coal mining), 13 (oil and gas extraction), 14 (mining and quarrying of non-metallic minerals), 20 (food and kindred products), 29 (petroleum refining and related industries), 40 (railroad transportation), 41 (transit, passenger transportation), 42 (motor freight transportation), 44 (water transportation), 45 (air transportation), 46 (pipelines, except natural gas), 48 (communications), 49 (electric, gas and sanitary services), 60 (depository institutions), 61 (nondepository credit institutions), 62 (security and commodity brokers, dealers), 63 (insurance carriers), 64 (insurance agents, brokers, and service), and 67 (holding and other investment offices).

[17] Also, based on Hogan and Jeter (1999), we classify firms in the following two-digit SIC codes as relatively high litigation-risk industries: 28 (chemicals and allied products), 35 (industrial and commercial machinery and computer equipment), 36 (electronic and other electrical equipment and components, except computer equipment), 38 (measuring, analyzing, and controlling instruments), 60 (depository institutions), 67 (holding and other investment offices), and 73 (business services).

[18] We use the *BIG* indicators to control for the effects of audit firm mergers and the demise of Arthur Andersen, which together reduced the number of large audit firms from eight to four during our test period. Our market share measures are sensitive to the number of auditors serving a market. For example, assuming equal market shares, in a market with eight auditors, each auditor would have a 12.5 percent market share, but in a market with five auditors, each auditor would have a 20 percent market share. Thus, our *BIG* indicators allow for different intercepts depending on the number of large auditors that were active at the time. Keeping in mind that an observation is defined at the industry rather than firm level, the *BIG* indicators capture the adjustment to the intercept, which represents the 2002–2004 period when there were only four large audit firms. Hence, we expect that each of the *BIG* indicators will have a negative coefficient because more large auditors in the marketplace should be associated with lower market shares on average, with the coefficient on *BIG8* being the most negative and the coefficient on *BIG5* being the least negative.

Data

Consistent with prior research (e.g., Kwon 1996; Hogan and Jeter 1999), we use three-digit SIC codes to classify industries. We include all firms on Compustat at the end of 2004, and we collect data for the period 1984 through 2004. Because some of our variables include lagged measures, our test period is 1986 through 2004. We first compute our *ACR2* measure for each industry-year with at least five firm-years of data for total assets and auditor. These computations involve 170,396 firm-year observations.

Next, at the firm level, we compute the four measures needed for the IOS factor analysis (i.e., prior investment intensity, geometric growth in market value of assets, ratio of market value to book value of assets, ratio of R&D expenditure to book value of assets) for all firm-years with sufficient data. We then estimate a factor analysis on these four firm-level variables using 90,894 firm-year observations.[19] For each industry-year with at least five firms with factor scores, we compute *IIOS* and *HIOS*.[20] This results in a sample of 3,443 industry-year observations over 19 years. We have a maximum of 200 industry-year observations in 1998 and a minimum of 162 in 1986. The median number of firms per industry for the 21-year period is 23.

Results

Table 1, Panel A provides descriptive statistics for variables used in our specialization tests. The mean (median) *ACR2* is 61.1 percent (60.5 percent), which indicates that the average market share for the two largest auditors is 61 percent across all industry-years. The first and third quartiles are 51.9 percent and 70 percent, respectively, and the standard deviation of *ACR2* is 13 percent. The mean and median of *IIOS* are negative, but because *IIOS* provides a relative rank of IOS across industries and is not an absolute measure, a negative *IIOS* measure does not imply negative investment opportunities.[21] The mean four-firm concentration ratio (*CCR4*) is 49.8 percent, and the mean of *MEANSIZE* is 23.331. Further, 23 percent of the industry-year observations are regulated, and 21 percent are from high-litigation-risk industries.

Table 2, Panel A provides pairwise correlations for our independent variables. The two IOS measures (*IIOS* and *HIOS*) are negatively correlated ($r = -0.472$). This suggests that as industry IOS increases, firms in the industry become less homogeneous. This is not surprising since greater industry IOS implies a greater number of investment strategies that a firm can pursue. The next highest correlation is between *IIOS* and *LITRISK* ($r = 0.411$), suggesting that growth opportunities are higher in high litigation industries. *IIOS* is negatively correlated with *CCR4* and *MEANSIZE*, which could reflect competition within an industry. More concentrated industries and large industries tend to be more mature industries where IOS is relatively lower.[22]

Table 3 identifies the ten industries ranked into the highest, middle, and lowest industries ranked by *IIOS* (left column) and *HIOS* (right column). We show the top, middle, and lowest ten industries for 2002 (the first Big 4 year) to give some sense of how industries rank.

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[19] We use the maximum number of firms to compute our specialization measure to obtain the most comprehensive measure of audit market structure. If we compute *ARC2* using only those firms with IOS data, then we ignore a large part of the market. Thus, our tests assume that the IOS for the firms with the data to compute the IOS measures is representative of the IOS for all firms in the same industry, consistent with the Smith and Watts (1992) assertion that there is a significant industry component to individual firms' IOS.

[20] We repeat our tests with a minimum of ten firms per industry with IOS data. Our results are qualitatively similar.

[21] By construction, the mean of the firm-level factor scores used to compute *IIOS* will be equal to zero across all firms in our sample. Also, note that the means and medians in Table 2 are based on equally weighted industries. For example, a large, growing industry with positive IOS is weighted the same as a small, declining industry with negative IOS.

[22] Table 1, Panel B and Table 2, Panel B are discussed in Section VI.

TABLE 1

Descriptive Statistics for Specialization, Dominance, and Audit Fee Tests

Panel A: Specialization and Dominance Tests (n = 3,443)

Variable	Mean (proportion of industries where measure = 1)	Std. Dev.	Quartile 1	Median	Quartile 3
ACR2	0.611	0.130	0.519	0.605	0.700
IIOS	−0.320	0.256	−0.478	−0.382	−0.235
HIOS	−0.554	0.429	−0.715	−0.425	−0.264
REG	(0.228)				
CCR4	0.498	0.203	0.354	0.495	0.643
LITRISK	(0.209)				
MEANSIZE	23.331	20.128	11.560	17.310	27.890
SISD	3.412	5.911	1.095	1.800	3.147
SICA	4.635	5.737	2.000	2.889	4.667

Panel B: Audit Fee Tests (n = 560)

Variable	Mean	Std. Dev.	Quartile 1	Median	Quartile 3
LNFEES	5.839	0.653	5.413	5.787	6.257
IIOS	−0.328	0.252	−0.474	−0.400	−0.276
HIOS	−0.528	0.418	−0.715	−0.412	−0.215
LNASSETS	5.523	1.114	4.772	5.428	6.195
CATA	0.484	0.164	0.383	0.517	0.610
DE	0.381	0.446	0.085	0.222	0.549
ROA	−0.057	0.201	−0.082	−0.004	0.034
QUICK	1.869	1.407	1.064	1.530	2.216
LNSUB	0.623	0.266	0.440	0.611	0.802
FOREIGN	0.104	0.111	0.017	0.062	0.159
LOSS	0.329	0.202	0.182	0.302	0.466
OPINION	0.522	0.237	0.353	0.524	0.694
YREND	0.627	0.232	0.500	0.636	0.800
BIG	0.588	0.268	0.400	0.625	0.800

Sample sizes are the number of industry-year observations where industry is based on three-digit SIC codes. The sample period is 1986–2004 in Panel A and 2001–2004 in Panel B.

Variable Definitions (Panel A):

$ACR2_{kt}$ = market share of two largest auditors in industry k (measured in terms of total client assets);

$IIOS_{kt}$ = median value of IOS in industry k where IOS is measured using a factor score based on Baber et al. (1996);

(continued on next page)

TABLE 1 (continued)

$HIOS_{kt}$ = $-1 \times$ (standard deviation of IOS in industry *k*) where IOS is measured using a factor score based on Baber et al. (1996);
REG_{kt} = 1 if industry *k* is regulated, and 0 otherwise;
$CCR4_{kt}$ = four-firm industry concentration ratio for industry *k*;
$LITRISK_{kt}$ = 1 if industry *k* is a high litigation-risk industry, and 0 otherwise;
$MEANSIZE_{kt}$ = mean size, measured by square root of total assets (measured in millions) of all firms in industry *k*;
$SISD_{kt}$ = $[(\Sigma_{i=1}^{I_{kt}}(J_{ikt}-\bar{J}_{kt})^2)/(I_{kt}-1)]^{1/2}$ where J_{it} is the number of clients served by audit firm *i* in industry *k*, $\bar{J}_k$ is the mean number of clients served by audit firms in industry *k*. I_k is the number of auditors in industry *k*, and *t* is a year subscript. and
$SICA_{kt}$ = $[(\Sigma_{i=1}^{I_{kt}}J_{ikt})/I_{kt}]$ where J_k is the number of clients served by audit firm *i* in industry *k*, I_t is the number of auditors in industry *k*, and *t* is a year subscript.

Additional Variable Definitions (Panel B):
$LNFEES_{kt}$ = the mean of the natural log of audit fees in industry *k*;
$LNASSETS_{kt}$ = mean of the natural log of total assets in industry *k*;
$CATA_{kt}$ = mean ratio of current assets to total assets in industry *k*;
DE_{kt} = mean of the ratio of long-term debt to common equity in industry *k*;
ROA_{kt} = mean return on assets in industry *k*;
$QUICK_{kt}$ = mean ratio of current assets less inventory to current liabilities in industry *k*;
$LNSUB_{kt}$ = mean of the natural log of the number of business segments in industry *k*;
$FOREIGN_{kt}$ = mean ratio of foreign sales to total sales in industry *k*;
$LOSS_{kt}$ = proportion of industry *k* firms that report a loss;
$OPINION_{kt}$ = proportion of industry *k* firms that report an unqualified opinion;
$YREND_{kt}$ = proportion of industry *k* firms that have a 12/31 fiscal year-end; and
BIG_{kt} = proportion of industry *k* firms that are audited by a Big N auditor.

While purely descriptive, the rankings provided in Table 3 are not at odds with our expectations. For example, the ten industries with the highest IOS measures include several high-tech and medical-related industries, and drugs (three-digit SIC 283) is the highest ranked industry based on *IIOS*. On the other hand, many of the industries ranked in the bottom 10 based on *IIOS* are related to financial services, insurance, natural resources, and transportation. In untabulated results, we also ranked industries in 1986 (first Big 8 year), 1989 (first Big 6 year), and 1998 (first Big 5 year). Classifications are quite stable, especially for the high IOS category. For example, six industries (i.e., communication equipment; computer and office equipment; drugs; laboratory apparatus and analytical, optical; research, development, and testing services; surgical, medical, and dental instruments) appear in the top 10 in at least three of the four years. Further, in all four years, drugs is the highest ranked industry. For the rankings based on *HIOS*, the top 10 industries (i.e., with more homogeneous industry IOS) appear to be mature, stable industries, e.g., paper and fabric mills, railroads, farm machinery, and grocery stores.

Table 4 summarizes the results from estimating Equation (1). Model (1) has an adjusted R^2 of 47.1 percent.[23] This is considerably higher than the 30 percent R^2 reported by Hogan and Jeter (1999), who include *ACR3*, no IOS variables, and no *BIG* indicators, and use data from 1976–1993. Consistent with H1, specialist auditors appear to be capturing a bigger share of those industries with higher levels of industry IOS, as evidenced by a positive relation between *IIOS* and *ACR2*. Also, we find a positive relation between *HIOS* and *ACR2*, suggesting that as the

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[23] In all tests, the variance inflation factors (VIF) are below 10, or the results are not sensitive to dropping any variable with a VIF above 10. The variable dropped for each such sensitivity test is *TIME*, which is correlated with *BIG8*, *BIG6*, and *BIG5*. Although values above 2.5 may be a cause of concern in logistic regressions, in OLS regressions it is generally only values of VIF over 10 that indicate multicollinearity (Lardaro 1993, 46).

TABLE 2

Pairwise Correlations for IOS and Control Variables for Specialization, Dominance, and Audit Fee Tests

Panel A: Specialization and Dominance Tests (n = 3,443)

	IIOS	_HIOS_	_TIME_	_REG_	_CCR4_	_LITRISK_	_MEANSIZE_
HIOS	−0.472***						
TIME	−0.111***	0.115***					
REG	_−0.094***_	_0.063**_	_0.014_				
CCR4	−0.163***	0.143***	0.087***	_−0.172***_			
LITRISK	_0.411***_	_−0.282***_	_0.002_	_−0.224***_	_−0.104***_		
MEANSIZE	−0.186***	0.231***	0.284***	_0.310***_	−0.052***	_−0.059***_	
ACR2	−0.133***	0.176***	0.369***	_0.025_	0.562***	_−0.071***_	0.246***

Panel B: Audit Fee Tests (n = 560)

	IIOS	_HIOS_	_LNASSETS_	_CATA_	_DE_	_ROA_
HIOS	−0.541***					
LNASSETS	−0.344***	0.412***				
CATA	0.320***	−0.223***	−0.452***			
_DE_	−0.174***	0.134***	0.275***	−0.397***		
ROA	−0.299***	0.461***	0.371***	−0.118***	0.040	
QUICK	0.439***	−0.423***	−0.413***	0.209***	−0.209***	−0.220***
LNSUB	−0.214***	0.172***	0.251***	−0.236***	0.173***	0.065
FOREIGN	0.074*	−0.127***	0.067	0.001	−0.201***	−0.007
LOSS	0.299***	−0.345***	−0.360***	0.097**	0.025	−0.505***
OPINION	−0.119***	0.029	−0.008	−0.036	0.060	−0.033
YREND	0.020	−0.104**	0.135***	−0.439***	0.108***	−0.077*
BIG	−0.226***	0.214***	0.528***	−0.232***	−0.169***	0.254***

	QUICK	_LNSUB_	_FOREIGN_	_LOSS_	_OPINION_	_YREND_
LNSUB	−0.200***					
FOREIGN	0.120***	0.144***				
LOSS	0.247***	−0.079*	0.008			
OPINION	−0.127***	0.001	−0.118***	0.014		
YREND	0.164***	0.213***	0.223***	0.159***	−0.076*	
BIG	−0.094**	0.121***	0.348***	−0.386***	−0.171***	0.120***

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*, **, *** Indicate significance at 0.10, 0.05, and 0.01 levels, respectively. Tests are two-tailed.

Sample sizes are the number of industry-year observations where industry is based on three-digit SIC codes. The sample period is 1986–2004 in Panel A and 2001–2004 in Panel B.

Correlations are Pearson or Spearman (Spearman shown in italics).

(continued on next page)

TABLE 2 (continued)

Variable Definitions (Panel A):

$IIOS_{kt}$ = median value of IOS in industry *k* where IOS is measured using a factor score based on Baber et al. (1996);
$HIOS_{kt}$ = −1 × (standard deviation of IOS in industry *k*) where IOS is measured using a factor score based on Baber et al. (1996);
$TIME_{kt}$ = linear time effect variable (=1,...,*T*);
REG_{kt} = 1 if industry *k* is regulated, and 0 otherwise;
$CCR4_{kt}$ = four-firm industry concentration ratio for industry *k*;
$LITRISK_{kt}$ = 1 if industry *k* is a high litigation-risk industry, and 0 otherwise; and
$MEANSIZE_{kt}$ = mean size, measured by square root of total assets (measured in millions) of all firms in industry *k*.

Additional Variable Definitions (Panel B):

$LNASSETS_{kt}$ = mean of the natural log of total assets in industry *k*;
$CATA_{kt}$ = mean ratio of current assets to total assets in industry *k*;
DE_{kt} = mean of the ratio of long-term debt to common equity in industry *k*;
ROA_{kt} = mean return on assets in industry *k*;
$QUICK_{kt}$ = mean ratio of current assets less inventory to current liabilities in industry *k*;
$LNSUB_{kt}$ = mean of the natural log of the number of business segments in industry *k*;
$FOREIGN_{kt}$ = mean ratio of foreign sales to total sales in industry *k*;
$LOSS_{kt}$ = proportion of industry *k* firms that report a loss;
$OPINION_{kt}$ = proportion of industry *k* firms that report an unqualified opinion;
$YREND_{kt}$ = proportion of industry *k* firms that have a 12/31 fiscal year-end; and
BIG_{kt} = proportion of industry *k* firms that are audited by a Big N auditor.

homogeneity of IOS in an industry increases, the market share of the two leading auditors increases. This supports H2, consistent with our knowledge transfer argument. The positive coefficient for *IIOS*HIOS* indicates that the effects of IOS levels and homogeneity interact positively, as expected, to increase the attractiveness of an industry for auditor specialization.

As indicated by the coefficients on our control variables and consistent with Hogan and Jeter (1999), auditor specialization increases with the regulatory status of the industry, firm concentration in the industry, and the mean size of firms in the industry. In our model, *TIME* is not significant because our *BIG* indicators subsume its explanatory power.[24]

We conduct two additional tests. First, since the number of firms in industries in our sample varies widely, we introduce an additional control for the number of industry members. *Ceteris paribus*, it is likely to be easier for an auditor to capture a large market share in industries with few firms than in industries with many firms since, in the former, an auditor may need only one or two large clients to capture a major share of the market. Consequently, we include a variable representing the number of firms in industry *k* in year *t* (*NFIRMS*) in Equation (1). The coefficient on *NFIRMS* is insignificant, but our results (untabulated) for *IIOS*, *HIOS*, and *IIOS*HIOS* are consistent with the results for model (1) in Table 4.

Second, we estimate Equation (1) without *CCR4* because of a concern that *CCR4* may be usurping explanatory power from *IIOS* and *HIOS*. Since the IOS affects a firm's investment and financing decisions, we expect that the IOS will affect the growth and distribution of firms in an industry. That is, *IIOS* and *HIOS* can be viewed as underlying determinants of *CCR4*. Table 4 provides the results for model (1A), where we omit *CCR4*. We estimate this model with *NFIRMS* for reasons stated above. Our results are robust to the inclusion or exclusion of *CCR4* and the

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[24] We also estimate our models with the *BIG* indicators omitted, and the coefficient on *TIME* is consistently positive and significant in that specification.

TABLE 3

Distribution of Industries by *IIOS* and *HIOS* in 2002

	Ranked by IIOS		Ranked by HIOS
SIC	Industry	SIC	Industry
		Bottom 10	
104	Gold and silver ores	782	Motion picture distribution and allied services
616	Mortgage bankers and brokers	366	Communications equipment
153	Operative builders	274	Miscellaneous publishing
635	Surety insurance	873	Research, development, and testing services
783	Motion picture theaters	351	Engines and turbines
501	Motor vehicles and motor vehicle parts	283	Drugs
499	Electric, gas, and sanitary services	202	Dairy products
633	Fire, marine, and casualty insurance	359	Miscellaneous industrial and commercial
452	Air transportation, nonscheduled	365	Household audio and video equipment, and audio
614	Personal credit institutions	874	Management and public relations services
		Middle 10	
329	Abrasive, asbestos, and miscellaneous	138	Oil and gas field services
267	Converted paper and paperboard products	122	Bituminous coal and lignite mining
363	Household appliances	734	Services to dwellings and other buildings
160	Heavy construction other than building construction contractors	517	Petroleum and petroleum products
531	Department stores	731	Advertising
371	Motor vehicles and motor vehicle equipment	800	Health services
504	Professional and commercial equipment	621	Security brokers, dealers, and flotation
232	Men's and boys' furnishings, work clothing	599	Retail stores, not elsewhere classified

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TABLE 3 (continued)

Ranked by IIOS			Ranked by HIOS
SIC	**Industry**	**SIC**	**Industry**
376	Guided missiles and space vehicles and parts	799	Miscellaneous amusement and recreation
302	Rubber and plastics footwear	208	Beverages
	Top 10		
355	Special industry machinery, except metalworking	204	Grain mill products
361	Electric transmission and distribution equipment	750	Automotive repair, services, and parking
873	Research, development, and testing services	493	Combination electric and gas, and other utility
367	Electronic components and accessories	401	Railroads
382	Laboratory apparatus and analytical, optical	655	Land subdividers and developers
737	Computer programming, data processing	265	Paperboard containers and boxes
366	Communications equipment	221	Broadwoven fabric mills, cotton
357	Computer and office equipment	352	Farm and garden machinery and equipment
351	Engines and turbines	541	Grocery stores
283	Drugs	451	Air transportation, scheduled, and air courier

TABLE 4

Association between Auditor Specialization and Industry IOS

		Model (1)		Model (1A)	
		Coefficient	**t-statistic**	**Coefficient**	**t-statistic**
Constant		0.503	27.81***	0.751	37.52***
IIOS	+	0.043	3.10***	0.078	4.83***
HIOS	+	0.021	3.93***	0.028	4.56***
*IIOS*HIOS*	+	0.020	1.87**	0.030	2.39***
TIME	+	−0.001	0.66	−0.001	−0.95
REG	+	0.017	3.99***	−0.002	−0.31
CCR4	+	0.352	41.68***		
LITRISK	−	0.003	0.76	0.002	0.32
MEANSIZE	+	0.001	11.91***	0.001	8.10***
BIG8	−	−0.138	−9.30***	−0.173	−9.99***
BIG6	−	−0.084	−8.74***	−0.114	−10.10***
BIG5	−	−0.041	−6.79***	−0.056	−7.84***
NFIRMS	−			−0.001	−17.76***
Adj. R^2		0.471		0.271	
F-statistic		280.16***		117.28***	
No. of obs.		3,443		3,443	

, * Indicate significance at 0.05 and 0.01 levels, respectively. Tests are one-tailed where a sign is predicted. Industries are based on three-digit SIC codes. The sample period is 1986-2004.

Model:

$$ACR2_{kt} = \beta_0 + \beta_1 IIOS_{kt} + \beta_2 HIOS_{kt} + \beta_3 IIOS_{kt}{*}HIOS_{kt} + \beta_4 TIME_{kt} + \beta_5 REG_{kt} + \beta_6 CCR4_{kt} + \beta_7 LITRISK_{kt} + \beta_8 MEANSIZE_{kt} + \beta_9 BIG8_{kt} + \beta_{10} BIG6_{kt} + \beta_{11} BIG5_{kt} \quad (1)$$

$$ACR2_{kt} = \beta_0 + \beta_1 IIOS_{kt} + \beta_2 HIOS_{kt} + \beta_3 IIOS_{kt}{*}HIOS_{kt} + \beta_4 TIME_{kt} + \beta_5 REG_{kt} + \beta_6 NFIRMS_{kt} + \beta_7 LITRISK_{kt} + \beta_8 MEANSIZE_{kt} + \beta_9 BIG8_{kt} + \beta_{10} BIG6_{kt} + \beta_{11} BIG5_{kt} \quad (1A)$$

Variable Definitions:

$ACR2_{kt}$ = market share of two largest auditors in industry *k* (measured in terms of total client assets);
$IIOS_{kt}$ = median value of IOS in industry *k* where IOS is measured using a factor score based on Baber et al. (1996);
$HIOS_{kt}$ = −1 × (standard deviation of IOS in industry *k*) where IOS is measured using a factor score based on Baber et al. (1996);
$TIME_{kt}$ = linear time effect variable (=1,...,*T*);
REG_{kt} = 1 if industry *k* is regulated, and 0 otherwise;
$CCR4_{kt}$ = four-firm industry concentration ratio for industry *k*;
$NFIRMS_{kt}$ = number of firms in industry *k*;
$LITRISK_{kt}$ = 1 if industry *k* is a high litigation-risk industry, and 0 otherwise;
$MEANSIZE_{kt}$ = mean size, measured by square root of total assets (measured in millions) of all firms in industry *k*;
BIG8 = 1 if year is 1986–1988, and 0 otherwise;
BIG6 = 1 if year is 1989–1997, and 0 otherwise; and
BIG5 = 1 if year is 1998–2001, and 0 otherwise.

t-statistics for *IIOS*, *HIOS*, and *IIOS*HIOS* all increase relative to model (1) as we expect, although the model's explanatory power falls to 27 percent.[25]

V. AUDITOR DOMINANCE—ANALYSIS AND RESULTS

Method

We use two measures of auditor dominance as proposed by Kwon (1996):

$$SISD_{kt} = \left[\left(\sum_{i=1}^{I_{kt}} (J_{ikt} - \bar{J}_{kt})^2 \right) \Big/ (I_{kt} - 1) \right]^{1/2}$$

$$SICA_{kt} = \left[\left(\sum_{i=1}^{I_{kt}} J_{ikt} \right) \Big/ I_{kt} \right]$$

where J_{ik} is the number of clients served by audit firm i in industry k, $\bar{J}_k$ is the mean number of clients served by audit firms in industry k, I_k is the number of auditors in industry k, and t is a year subscript. *SISD* is a measure of the dispersion of auditors in an industry. If all auditors serve an equal number of clients in an industry, then *SISD* will equal zero. As an auditor becomes more dominant and captures more clients in the industry, *SISD* will increase. *SICA* measures the average number of clients served by auditors in an industry. If each auditor audits only one client, then *SICA* will have a value of 1. As an auditor becomes more dominant and captures additional clients, the number of clients in the industry remains the same, but the number of auditors servicing the industry will decrease, resulting in an increase in *SICA*.

To test our dominance hypothesis (H3), we adapt Kwon's (1996) model by adding our IOS variables, *TIME* variable, and *BIG* indicators. Our models are:

$$SISD_{kt} = \gamma_0 + \gamma_1 IIOS_{kt} + \gamma_2 HIOS_{kt} + \gamma_3 IIOS_{kt} {*} HIOS_{kt} + \gamma_4 REG_{kt} + \gamma_5 CCR4_{kt} + \gamma_6 ACR2_{kt} + \gamma_7 TIME_{kt} + \gamma_8 BIG8_{kt} + \gamma_9 BIG6_{kt} + \gamma_{10} BIG5_{kt} \quad (2)$$

$$SICA_{kt} = \gamma_0 + \gamma_1 IIOS_{kt} + \gamma_2 HIOS_{kt} + \gamma_3 IIOS_{kt} {*} HIOS_{kt} + \gamma_4 REG_{kt} + \gamma_5 CCR4_{kt} + \gamma_6 ACR2_{kt} + \gamma_7 TIME_{kt} + \gamma_8 BIG8_{kt} + \gamma_9 BIG6_{kt} + \gamma_{10} BIG5_{kt} \quad (3)$$

IIOS, *HIOS*, *IIOS*HIOS*, *REG*, *CCR4*, *ACR2*, *TIME* and the *BIG* variables are as defined previously. *ACR2* is included to control for auditor specialization.[26]

Based on H3, we expect that γ_2 will be negative. Also, from H1, we expect γ_1 to be positive since higher industry IOS should make the industry more attractive for specialization. This suggests that holding homogeneity constant, high IOS industries would be more likely to have a dominant auditor. Finally, we expect that γ_3 will be negative since clients would be most concerned about proprietary information transfers in a setting where both levels and homogeneity of IOS are high.

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[25] The results tabulated in Table 4 are not sensitive to using either NAICS codes or Fama and French (2007) industry classifications in place of three-digit SIC codes.

[26] We also estimate our models including Kwon's (1996) *SPEC* variable, and our results are qualitatively unchanged. *SPEC* is the industry mean of the square root of total assets of clients from industry k for auditor i divided by the square root of total assets of clients of auditor i.

Data

Using Compustat data from 1984–2004, we calculate *SISD* and *SICA* for each three-digit SIC code industry. We then match these data to our existing sample. Our sample for the dominance analysis consists of the same 3,443 industry-year observations as used in the specialization tests.

Results

Table 1, Panel A provides descriptive statistics for the variables used in Equations (2) and (3). The mean for *SISD* is 3.412, compared to 1.83 reported by Kwon (1996, Table 4) for three-digit SIC code industries. The mean for *SICA* is 4.635, compared to 2.49 reported by Kwon (1996, Table 4). Taken together, these trends imply that the mergers among the former Big 8 firms led to increased auditor dominance and a greater average number of clients served over time.

Table 2, Panel A contains the pairwise correlations among the explanatory variables in Equations (2) and (3).[27] Table 5 presents the results of estimating Equations (2) and (3). Recall that the dependent variable for Equation (2) is *SISD*, while the dependent variable for Equation (3) is *SICA*. We find support for H3 in both models, i.e., auditor dominance is negatively related to the homogeneity of industry IOS. This is consistent with an argument that, as clients in an industry become increasingly similar in terms of their IOS, they are more concerned about the transfer of proprietary information, and auditor dominance decreases. While auditors should not intentionally disclose such information, the fact that it is in their possession increases the chance of an inadvertent slip with respect to relative performance or position within the industry.

The positive coefficients on IIOS provide further support for H1. When homogeneity of industry IOS is held constant, the industry becomes more attractive for auditor specialization as IOS levels increase, thus enhancing the appeal for a single auditor to try to dominate the industry. In contrast, *IIOS*HIOS* has a significant negative coefficient, indicating that the negative relation between *SISD* (*SICA*) and *HIOS* intensifies as *IIOS* increases. From the clients' perspective, there is greater aversion to sharing auditors when there is more proprietary information to be protected *and* more intense intra-industry competition. This finding indicates that levels and homogeneity of industry IOS have a joint effect on auditor dominance.

Our models yield significantly negative coefficients for *CCR4*, suggesting that it is harder for an auditor to dominate a highly concentrated industry. We find evidence of a positive relation between *ACR2*, our measure of specialization, and *SISD* in model (2), and *SICA* in model (3). Thus, there is evidence that industries with more intensive specialization by auditors are also more likely to have a dominant auditor after controlling for *IIOS* and *HIOS*. Taken together, our results suggest that industry IOS levels and homogeneity are determinants of auditor dominance, both individually and jointly.[28]

VI. AUDIT FEES—ANALYSIS AND RESULTS

Data

We use audit fee data from Audit Analytics to calculate audit firm market shares based on three-digit SIC codes for the years 2001 through 2004. We then match data from Compustat to

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[27] Note that *IIOS* and *ACR2* are negatively correlated, consistent with lower market shares for the top two auditors in industries with high IOS. This seems contrary to H1, which predicts a positive relation between *IIOS* and *ACR2*; however, these univariate correlations do not control for other factors.

[28] A limitation of the Compustat database is that it does not uniquely identify all non-Big auditors. In particular, depending on the year, some or all of the non-Big auditors are combined into an "other" category. To consider whether this affects our results, we re-estimate our dominance equations using data from Audit Analytics. Unlike Compustat, Audit Analytics uniquely identifies every non-Big auditor, but the Audit Analytics data are only available for 2001–2004. The results using the Audit Analytics data are consistent with our reported results.

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TABLE 6
Association between Audit Fees and Industry IOS

		Coefficient	t-statistic
Constant		2.123	17.66***
IIOS	+	0.374	3.64***
HIOS	+	0.086	2.11**
*IIOS*HIOS*	+	0.273	3.23***
LNASSETS	+	0.483	33.69***
CATA	+	0.935	11.44***
DE	+/−	−0.019	−0.66
ROA	−	−0.202	−3.27***
QUICK	−	−0.038	−4.25***
LNSUB	+	0.203	4.94***
FOREIGN	+	0.367	3.55***
LOSS	+	0.207	3.29***
OPINION	−	0.028	0.49
YREND	+	0.288	5.47***
BIG	+	0.117	1.61*
YR2002	+/−	0.210	6.23***
YR2003	+/−	0.289	6.85***
YR2004	+/−	0.736	15.90***
Adj. R^2		0.873	
F-statistic		227.15***	
No. of obs.		560	

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*, **, *** Indicate significance at 0.10, 0.05, and 0.01 levels, respectively. Tests are one-tailed where a sign is predicted. Industries are based on three-digit SIC codes. The sample period is 2001–2004.

Model:

$$\begin{aligned} LNFEES_{kt} = {} & \varphi_0 + \varphi_1 IIOS_{kt} + \varphi_2 HIOS_{kt} + \varphi_3 IIOS{*}HIOS_{kt} + \varphi_4 LNASSETS_{kt} + \varphi_5 CATA_{kt} + \varphi_6 DE_{kt} + \varphi_7 ROA_{kt} \\ & + \varphi_8 QUICK_{kt} + \varphi_9 LNSUB_{kt} + \varphi_{10} FOREIGN_{kt} + \varphi_{11} LOSS_{kt} + \varphi_{12} OPINION_{kt} + \varphi_{13} YREND_{kt} + \varphi_{14} BIG_{kt} \\ & + \varphi_{15} YR2002_{kt} + \varphi_{16} YR2003_{kt} + \varphi_{17} YR2004_{kt} \end{aligned} \tag{4}$$

Variable Definitions:

$LNFEES_{kt}$ = the mean of the natural log of audit fees in industry *k*;
$IIOS_{kt}$ = median value of IOS in industry *k* where IOS is measured using a factor score based on Baber et al. (1996);
$HIOS_{kt}$ = −1 × (standard deviation of IOS in industry *k*) where IOS is measured using a factor score based on Baber et al. (1996);
$LNASSETS_{kt}$ = mean of the natural log of total assets in industry *k*;
$CATA_{kt}$ = mean ratio of current assets to total assets in industry *k*;
DE_{kt} = mean of the ratio of long-term debt to common equity in industry *k*;
ROA_{kt} = mean return on assets in industry *k*;
$QUICK_{kt}$ = mean ratio of current assets less inventory to current liabilities in industry *k*;
$LNSUB_{kt}$ = mean of the natural log of the number of business segments in industry *k*;
$FOREIGN_{kt}$ = mean ratio of foreign sales to total sales in industry *k*;

(continued on next page)

TABLE 6 (continued)

$LOSS_{kt}$ = proportion of industry *k* firms that report a loss;
$OPINION_{kt}$ = proportion of industry *k* firms that report an unqualified opinion;
$YREND_{kt}$ = proportion of industry *k* firms that have a 12/31 fiscal year-end;
BIG_{kt} = proportion of industry *k* firms that are audited by a Big N auditor; and
YR2002 (YR2003, YR2004) = indicator variable that is equal to 1 if the year is 2002 (2003, 2004, respectively).

We proceed to consider whether the higher fees observed for industries with relatively high and homogeneous IOS are more likely to be attributable to market power by auditors in such industries or to successful product differentiation. Following Simunic (1980), who devised a model in which an intra-industry comparison of fees between large and small auditees served as a test for competition, we next partition each industry in our sample into large and small clients, relative to the median client (based on total assets) in that industry. As alleged in Simunic (1980), the market for audit services may be viewed as a hedonic market (Rosen 1974), where an unobservable differentiated product is revealed by differences in prices. Thus, higher fees for both large and small clients, related in our case to the levels and homogeneity of industry IOS, could be interpreted as evidence of product differentiation by auditors of these firms.

On the other hand, higher prices for large clients only would be consistent with monopolistic behavior by the auditors in this market segment. Large clients have reduced choice due to their need for an auditor that is large enough to be independent of its individual clients, in addition to market pressures to hire a highly reputed auditor and the need to protect proprietary information. In contrast, smaller clients in a given industry are less likely to be exposed to such pressures, and may be less likely to be swayed by the desire to protect proprietary information if they do not consider themselves to be viewed as a significant threat to the bigger players in the industry. Thus, if small clients are also paying increased fees in the high *IIOS* or *HIOS* industries, then this would indicate that the fees are more consistent with product differentiation than with auditor power. Finally, if we observe that the association between fees and IOS is not significant for large firms in our sample but is significant for small firms, then we would conclude that large firms are able to use their bargaining power to negotiate away the fee premiums attributable to product differentiation, whereas smaller firms are unable to do so.

We recompute all our variables for the large and small client segments separately and re-estimate Equation (4) for each subsample. Table 7 provides the results for the two subsamples. For the small client subsample, *IIOS* and *IIOS*HIOS* are significant ($p < 0.01$) and positively signed. For the large client subsample, *IIOS*, *HIOS*, and *IIOS*HIOS* are significant (at $p < 0.10$, $p < 0.10$, and $p < 0.05$, respectively) and positively signed. Thus, Table 7 suggests there is not much difference between the two subsamples as far as the IOS variables are concerned. While we would not claim that these findings are conclusive, we contend that they are, at least, consistent with an argument that the higher fees documented for high and homogeneous IOS industries are more likely attributable to product differentiation than to audit firms exercising market power.[29]

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[29] We also divide the sample according to the percentage of each industry's clients that use Big N auditors since a low (high) percentage may reflect a more (less) competitive market for auditors. We obtain qualitatively similar results to those reported in Table 7, i.e., we find further support for product differentiation.

TABLE 7

Association between Audit Fees and Industry IOS for Small and Large Subsamples

		Small Client Subsample		Large Client Subsample	
		Coefficient	**t-statistic**	**Coefficient**	**t-statistic**
Constant		2.501	20.901	2.413	14.501
IIOS	+	0.435	3.712***	0.224	1.568*
HIOS	+	0.056	1.220	0.078	1.408*
*IIOS*HIOS*	+	0.229	2.389***	0.243	2.092**
LNASSETS	+	0.424	26.757***	0.460	28.020***
CATA	+	0.693	8.398***	0.797	7.254***
DE	+/−	0.035	1.196	−0.009	−0.329
ROA	−	0.000	0.453	−0.186	−0.708
QUICK	−	−0.029	−4.546***	−0.045	−3.991***
LNSUB	+	0.124	2.595***	0.256	5.992***
FOREIGN	+	−0.083	−0.515	0.714	6.618***
LOSS	+	0.387	6.951***	0.307	3.236***
OPINION	−	−0.012	−0.225	−0.062	−0.902
YREND	+	0.161	3.165***	0.149	2.395***
BIG	+	0.389	5.813***	0.017	0.204
YR2002	+/−	0.162	4.605***	0.202	4.174***
YR2003	+/−	0.164	3.400***	0.328	6.497***
YR2004	+/−	0.514	10.419***	0.865	15.187***
Adj. R^2		0.824		0.785	
F-statistic		154.441		120.957	
No. of obs.		560		560	

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*, **, *** Indicate significance at 0.10, 0.05, and 0.01 levels, respectively. Tests are one-tailed where a sign is predicted. Industries are based on three-digit SIC codes. The sample period is 2001–2004. The sample is partitioned based on the median of total assets in each industry.

Model:

$$LNFEES_{kt} = \varphi_0 + \varphi_1 IIOS_{kt} + \varphi_2 HIOS_{kt} + \varphi_3 IIOS{*}HIOS_{kt} + \varphi_4 LNASSETS_{kt} + \varphi_5 CATA_{kt} + \varphi_6 DE_{kt} + \varphi_7 ROA_{kt} + \varphi_8 QUICK_{kt} + \varphi_9 LNSUB_{kt} + \varphi_{10} FOREIGB_{kt} + \varphi_{11} LOSS_{kt} + \varphi_{12} OPINION_{kt} + \varphi_{13} YREND_{kt} + \varphi_{14} BIG_{kt} + \varphi_{15} YR2002_{kt} + \varphi_{16} YR2003_{kt} + \varphi_{17} YR2004_{kt} \quad (4)$$

Variable Definitions:

$LNFEES_{kt}$ = the mean of the natural log of audit fees in industry *k*;
$IIOS_{kt}$ = median value of IOS in industry *k* where IOS is measured using a factor score based on Baber et al. (1996);
$HIOS_{kt}$ = −1× (standard deviation of IOS in industry *k*) where IOS is measured using a factor score based on Baber et al. (1996);
$LNASSETS_{kt}$ = mean of the natural log of total assets in industry *k*;
$CATA_{kt}$ = mean ratio of current assets to total assets in industry *k*;
DE_{kt} = mean of the ratio of long-term debt to common equity in industry *k*;
ROA_{kt} = mean return on assets in industry *k*;
$QUICK_{kt}$ = mean ratio of current assets less inventory to current liabilities in industry *k*;

(continued on next page)

TABLE 7 (continued)

$LNSUB_{kt}$ = mean of the natural log of the number of business segments in industry k;
$FOREIGN_{kt}$ = mean ratio of foreign sales to total sales in industry k;
$LOSS_{kt}$ = proportion of industry k firms that report a loss;
$OPINION_{kt}$ = proportion of industry k firms that report an unqualified opinion;
$YREND_{kt}$ = proportion of industry k firms that have a 12/31 fiscal year-end;
BIG_{kt} = proportion of industry k firms that are audited by a Big N auditor; and
YR2002 (YR2003, YR2004) = indicator variable that is equal to 1 if the year is 2002 (2003, 2004, respectively).

Taken together, our findings imply that the highest premiums are likely to accrue to specialists in high *IIOS* and high *HIOS* industries. In summary, our findings with respect to the IOS and its relation to specialization, dominance, and fees suggest that it is economically beneficial for auditors to pursue specialization in high *IIOS* and high *HIOS* industries.

VII. SENSITIVITY ANALYSIS

We conduct several sensitivity tests to check the robustness of our results. First, recognizing that *ACR2* is an imperfect measure of industry specialization, we replicate our industry specialization analyses using two alternative specialization definitions based on Cairney and Young (2006). Drawing on Neal and Riley (2004), Cairney and Young (2006) develop a composite measure of specialization that reflects both the auditor's market share and the importance of an industry in the auditor's portfolio. We label this variable *PFOLIO*:

$$PFOLIO_{ikt} = \left[\left(\sum_{j=1}^{J_{ikt}} \sqrt{A_{ijkt}}\right) \Big/ \left(\sum_{k=1}^{K_t} \sum_{j=1}^{J_{ikt}} \sqrt{A_{ijkt}}\right)\right] * \left[\left(\sum_{j=1}^{J_{ikt}} \sqrt{A_{ijkt}}\right) \Big/ \left(\sum_{i=1}^{I_{kt}} \sum_{j=1}^{J_{ikt}} \sqrt{A_{ijkt}}\right)\right]$$

where J_{ik} is the number of clients served by audit firm i in industry k, I_k is the number of auditors in industry k, K is the number of industries, A_{ijk} represents the total assets of client firm j in industry k audited by auditor i, and t is a year subscript.

Large portfolio shares reflect significant investments by audit firms in developing industry audit technologies (Neal and Riley 2004). Evidence of a positive association between industry IOS measures and auditor industry portfolio shares suggests that individual audit firms are attracted to industries where they are able to develop expertise and spread their production costs. We use a sample of 17,423 auditor-industry-year observations,[30] and we regress *PFOLIO* on *IIOS*, *HIOS*, *IIOS*HIOS* and the control variables from models (1) and (1A).[31] The coefficients on *HIOS* and *IIOS*HIOS* are positive and significant, but the coefficient on *IIOS* is not statistically significant.

We interpret the results of this sensitivity test as evidence that auditors' portfolio shares increase as industry IOS increases, but only when there is increasing homogeneity. Thus, when viewed at the auditor portfolio level, an auditor will choose to specialize in a high IOS industry only if there is enough homogeneity to allow the auditor to spread its costs of acquiring the relevant expertise and technology across multiple clients.

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[30] We start with the client-level observations described in our other tests.

[31] The sample consists of auditor-industry-year observations for Big auditors only. We do not use non-Big auditors for two reasons. First, Compustat does not provide the identity of all non-Big auditors for all years (i.e., in some years all non-Big auditors are treated as "other"). Second, Compustat does not provide data for private firms, potentially biasing the measure of the auditor's portfolio of firms in each industry. Consistent with the industry-level results reported in Table 4, we exclude industries with fewer than five members. The results are not sensitive to changing this restriction to a minimum of 15 industry members (the sample size becomes 13,384 in this case).

In our second alternative measure for specialization, we attempt to remove the effects of industry structure from our examination of the relation between *ACR2* and industry IOS. Once more following Cairney and Young (2006), we obtain the residual from regressing *ACR2* on *CCR4* and *NFIRMS*, and use it as the dependent variable in a model of *IIOS*, *HIOS*, *IIOS*HIOS*, *TIME*, *REG*, *LITRISK*, *MEANSIZE*, *BIG8*, *BIG6*, and *BIG 5*. Our results are qualitatively unchanged from Table 4, providing additional support for H1 and H2. Thus, our results are robust to these alternative measures of specialization, although for our portfolio measure, H1 is supported only when *IIOS* is interacted with *HIOS*.

Our second set of sensitivity tests relates to our IOS measure. Since there is no definitive measure of IOS in the literature, we re-estimate our models using an alternative estimate of IOS adapted from Gaver and Gaver (1993). For this measure, we use the score from the factor analysis of: (1) the ratio of the market value to book value of total assets at the end of year *t*, (2) the ratio of research and development expenditure to book value of assets at the end of year *t*, (3) the ratio of the market value to book value of equity at the end of year *t*, and (4) the earnings to price ratio (EP). Our results using the Gaver and Gaver (1993) measures are qualitatively similar to our principal results, although there is some loss of power as a few industries drop out of our sample due to extra data requirements for these measures.

To examine whether our results are sensitive to our definition of central tendency, in our third set of sensitivity tests we re-estimate our models using the mean, rather than the median, of IOS for firms in an industry to measure *IIOS*. While the explanatory power and significance levels vary slightly, the results are qualitatively consistent across all of the specialization, dominance and audit fees tests.

Our final sensitivity tests apply our models for individual years and for alternative groups of years. Again, the results are qualitatively similar to those presented. While a few variables lose a level of significance for some subperiods, it appears that our temporal control variables, *TIME* and *BIG8*, *BIG6*, and *BIG5*, serve their control purpose as there is no systematic pattern to year-specific variations. Thus, our results do not appear to be driven by any specific subperiod.

VIII. CONCLUSION

We examine the role that the investment opportunity set plays in determining whether an industry is an attractive target for auditor specialization, and we explore how the IOS affects auditor dominance and audit pricing. In doing so, we link two separate strands of literature, one examining industry specialization by auditors and the other examining the effects of the IOS. We posit that industries with higher IOS levels and homogeneity will create environments that are conducive to specialization. We also predict that the homogeneity of industry IOS will affect the ability of an auditor to dominate an industry and present evidence consistent with that prediction. In addition, we advance the literature on audit pricing by specialists, which has produced somewhat conflicting findings to date (e.g., Craswell et al. 1995; Ferguson and Stokes 2002; Casterella et al. 2004). We argue that both the level and homogeneity of industry IOS will affect the auditor's production costs and/or demand for specialist auditors, thereby affecting audit fees.

Using industry-level data, we present evidence that auditor concentration, our main proxy for industry specialization, is positively related to both the level and the homogeneity of industry IOS. These results also hold across other specialization measures. Further, our results show that the homogeneity of industry IOS is negatively related to measures of auditor dominance, and that both industry IOS levels and homogeneity are positively associated with audit fees. Additionally, we find that the higher fees are likely to be attributable to successful product differentiation rather than market power held by auditors in these industries. Overall, our results suggest that industry IOS affects the structure of industry audit markets and the fees charged by auditors in those markets.

Throughout this study we develop and test a theory linking the structure of audit markets to industry IOS as a means to expand our current understanding of the market for audit services. In doing so, we generally work within the constraints of existing models and proxy measures for dependent and independent variables such as auditor specialization and dominance. For example, while we consider alternative proxies for capturing industry specialization, we acknowledge that all these measures are imperfect, and as such, represent a limitation in interpreting our findings. A challenge for future research is to develop alternative measures of these concepts and to examine their construct validity in the context of this and other studies. At an even more fundamental level, future research could investigate the relative roles of auditors, independent directors, and other governance mechanisms in industries with different IOS features. Another potential extension of our study is to develop firm-level models that relate individual firms' IOS to their auditor choices and to the audit fees paid.

Overall, our findings contribute not only to the academic literature, but also offer some new insights into the GAO's concerns about auditor specialization. For instance, with respect to the GAO's suggestion to evaluate "ways to increase accounting firm competition in certain industries by limiting market shares" (GAO 2003, 53), our analysis suggests that industry specialization is a rational response to industry-specific knowledge requirements.

APPENDIX

Control Variables for Equation (4):

LNASSETS = mean of the natural log of total assets in industry *k*;
CATA = mean ratio of current assets to total assets in industry *k*;
DE = mean of the ratio of long-term debt to common equity in industry *k*;[32]
ROA = mean return on assets in industry *k*;
QUICK = mean ratio of current assets less inventory to current liabilities in industry *k*;
LNSUB = mean of the natural log of the number of business segments in industry *k*;
FOREIGN = mean ratio of foreign sales to total sales in industry *k*;
LOSS = proportion of industry *k* firms that report a loss;
OPINION = proportion of industry *k* firms that report an unqualified opinion;
YREND = proportion of industry *k* firms that have a 12 / 31 fiscal year-end;
BIG = proportion of industry *k* firms that are audited by a Big N auditor.

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[32] Because of extreme outliers, we winsorize *DE* at the 1 and 99 percentiles. However, our results are similar when the unwinsorized values are used.

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Matching and the Changing Properties of Accounting Earnings over the Last 40 Years

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ABSTRACT: We present a theory that poor matching manifests as noise in the economic relation of advancing expenses to earn revenues. As a result, poor matching decreases the correlation between contemporaneous revenues and expenses, increases earnings volatility, decreases earnings persistence, and induces a negative autocorrelation in earnings changes. The empirical tests document these effects in a sample of the 1,000 largest U.S. firms over the last 40 years. We find a clear and economically substantial trend of declining contemporaneous correlation between revenues and expenses, increased volatility of earnings, declining persistence of earnings, and increased negative autocorrelation in earnings changes. The combined evidence suggests that accounting matching has become worse over time and that this trend has a pronounced effect on the properties of the resulting earnings. This evidence also suggests that the standard-setters' stated goal of moving away from matching and toward more fair-value accounting is likely to continue and deepen the identified trends in the properties of earnings.

Keywords: *matching principle; fair-value accounting; earnings properties.*

I. INTRODUCTION

This study investigates the effects of poor matching on the properties of accounting earnings over the last 40 years. This topic is interesting for three reasons. First, earnings is the single most important output of the accounting system (Graham et al. 2005) and the matching of expenses to revenues has great impact on the determination of earnings. Therefore, an inquiry into matching could potentially provide valuable insights into the properties of accounting earnings. Second, since the late 1970s accounting standards have taken a deliberate and far-reaching turn away from matching as the fundamental concept in the determination of earnings and toward a more balance-sheet-based model of the determination of income. It is important to document and

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assess the consequences of this evolution for the properties of earnings. Third, changes in the real economy toward more fixed costs and R&D-type activities also imply a temporal decline in matching success, with corresponding effects on the properties of earnings. The temporal confluence of economic and accounting factors suggests there could be substantial changes in the properties of earnings over time, and it is important to document the economic magnitude of these changes.

The main ideas of the paper are formalized in a simple model of matching, where accounting earnings are shaped by both economic factors and the degree of matching success. The essence of the model is that mismatched expenses act as noise in the economic relation of advancing expenses to earn revenues, and therefore poor matching decreases the contemporaneous correlation between revenues and expenses. As a result, the mismatch between revenues and expenses increases the volatility of earnings, decreases the persistence of earnings, and increases the negative autocorrelation in earnings changes. Since poor matching resolves over time, we expect that the identified effects of poor matching are less pronounced for longer-horizon definitions of earnings.

We examine these predictions in a time-series framework because the evolution of standard-setting and the real economy provide strong reasons to believe that matching has become worse over time and, therefore, we expect to see detectable effects in the time-series properties of revenues, expenses, and earnings. We use a sample of the largest 1,000 firms over the last 40 years, and find that all predictions of the model are borne out in the data. The contemporaneous correlation between revenues and expenses has substantially decreased over time, while the correlation between current revenues and past and future expenses has increased. Earnings volatility has nearly doubled while the underlying volatilities of revenues and expenses have remained roughly the same. Earnings persistence has substantially declined from 0.91 to 0.65 during this period. The reversibility (or autocorrelation) of earnings changes is nearly zero at the beginning and reaches a surprisingly large −0.30 at the end of the period. We find more muted evidence that consideration of longer-horizon earnings attenuates the effects of poor matching, most probably because our longer-horizon specification is limited to two-year definitions of variables.

A battery of additional tests extends and solidifies the main results. First, we find that the same pattern of results exists after controlling for the well-known large temporal increase in the frequency of one-time items and losses. Second, we document that the same pattern of results obtains within industries that either increase or decrease in prominence over time, which suggests that industry composition and, more broadly, the changing characteristics of the sample firms and the real economy are unlikely to fully account for our findings. Third, in contrast to the pronounced temporal patterns in reported accrual-based measures, we find none of those temporal patterns in cash-based measures of revenues, expenses, and earnings. This evidence suggests that changing accounting has much to do with the identified trends in our main results.

These findings have a number of implications and can be a useful guide for further research. On the more immediate and obvious level, these results suggest that a consideration of matching generates useful insights into the properties of earnings, which can be helpful in pursuing topics such as the improved prediction of earnings and accounting-based valuation. For example, one can use historical patterns of correlations between revenues and expenses as a measure of matching success and as a predictor of earnings persistence to pursue quality of earnings investigations, possibly including earnings forecasting and equity valuation.

The results of this study can also be useful for more general and philosophical considerations. For example, if investors use reported earnings primarily to assess firms' long-run profitability, the temporal trends identified in this study suggest that earnings have become considerably less useful in that regard over the last 40 years. In addition, the prevailing philosophy and the specific current agenda of both the FASB and the IASB suggests that this trend is going to continue, and thus a future in which reported earnings have evolved into something which is divorced from their classic role as a gauge of long-term value looks highly probable in the next 30 to 50 years. Such an evolution would likely impose a considerable difficulty and cost on investors, and it is reasonable to suggest that standard-setters should more explicitly consider this cost in their deliberations.

The remainder of the paper is organized as follows. Section II presents the theory of the paper. Section III presents the main empirical tests and results. Section IV presents robustness checks and additional results. Section V provides a discussion of the results and suggestions for future research. Section VI concludes.

II. THEORY

We develop a simple model of matching that formalizes the link between poor matching and the properties of accounting earnings. It is important to clarify from the beginning that this paper takes a specific perspective on the determination of accounting earnings and that other perspectives are also possible and useful depending on the goals of the investigation. We view the firm as an entity that continually advances expenses hoping to reap revenues and earnings. Thus, firm success is naturally measured by the excess of revenues over the expenses advanced to earn it, and the goal of accounting matching is to explicitly recognize and measure this business reality. Poor matching is defined as the extent to which expenses are not matched against the resulting revenues, and is modeled as noise in the economic relation of advancing expenses to earn revenues.

Note that poor matching can arise for several reasons including unavoidable business factors (e.g., fixed costs, poor traceability of costs), managerial discretion (e.g., taking a big bath), and accounting rules (e.g., R&D is required to be expensed regardless of traceability). In addition, note that accounting practice differentiates between several degrees of matching success, i.e., "direct matching" for costs that are directly and specifically matched to associated revenue (cost of goods sold), "indirect matching" for costs that are matched indirectly by allocating them to periods (depreciation, taxes), and "no matching" or "expense as incurred" for costs where matching considerations are entirely absent (most R&D and advertising). At the theoretical level, we do not make a specific provision for the multitude of sources and manifestations of poor matching to keep things simple and because for our purposes all these aspects of poor matching have similar consequences on the properties of revenues, expenses, and especially earnings. In the empirical tests later in the paper, we provide some evidence on the relative importance of various sources and manifestations of poor matching.

Perfect Matching

We start the development of the model with the consideration of a benchmark case of "perfect matching," which is later contrasted with the case of "poor matching." Perfect matching is defined as the situation where all relevant expenses are matched against the associated revenue. In practical terms, perfect matching can be viewed as a case where all costs are directly and specifically traceable to specific revenues, e.g., a firm where the only costs are cost of goods sold and sales commissions. For the purposes of this paper, revenue recognition is largely treated as fixed and, therefore, the model treats the schedule of revenues as given and concentrates on the properties of matching and expense recognition. Accounting variables derived under perfect matching are denoted by an asterisk (*) throughout the paper. The earnings relations for firms with perfect matching are assumed to be given by the following expressions:

$$E_t^* = Rev_t^* - Exp_t^* \tag{1}$$

$$E_t^* = E_{CC} + \beta_1^*(E_{t-1}^* - E_{CC}) + \varepsilon_t \tag{2}$$

where E_t denotes earnings at time t, Rev and Exp denotes revenues and expenses, $0 < \beta_1^* < 1$, and

E_{CC} is the earnings that is given by the firm's cost of equity capital, and can be also thought of as the firm's required rate of return and the long-run mean of its earnings. The model assumes that E_{CC} is constant; however, the main conclusions remain the same if E_{CC} is varying over time or across firms.

On one level, Equation (1) is simply a statement of the income statement identity, that earnings is the residual of revenues minus all expenses. However, note that E_t^* in Equation (1) captures a notion of economic performance because it summarizes the results of spending Exp_t^* to get Rev_t^*. Thus, E_t^* in Equation (1) is our representation of "economic performance" or "economic earnings." The goal of Equation (2) is to provide a parsimonious representation of the essential time-series features of the economic earnings process. We choose a time-series specification because matching is essentially a time-series phenomenon, where all mismatching of expenses eventually resolves over time. Equation (2) incorporates several realistic and research-vetted time-series features of economic earnings. First, earnings arise in a competitive equilibrium, where long-run earnings tend to gravitate to the cost of equity capital. Second, recent deviations in earnings from the long-run mean tend to persist but gradually diminish over time (Freeman et al. 1982), so the β_1^* coefficient is positive but less than 1. Third, each period there is an economic shock, which is modeled as a mean-zero white noise. The variance of ε, denoted as $Var(\varepsilon)$, is a representation of the "economic volatility" in the firm's business environment.

Note that while the basic model in Equation (2) is presented in a way that clarifies the economic properties of earnings, it can be re-written in a more familiar way as:

$$E_t^* = (1 - \beta_1^*)E_{CC} + \beta_1^* E_{t-1}^* + \varepsilon_t.$$

Or, having in mind that E_{CC} and β_1^* are constants, and denoting $\beta_0^* = (1 - \beta_1^*)E_{CC}$ yields:

$$E_t^* = \beta_0^* + \beta_1^* E_{t-1}^* + \varepsilon_t. \tag{3}$$

Expression (3) is identical to a regression of present on past earnings, and is later used to establish the link between the quality of matching and earnings persistence.

The benchmark case of perfect matching in Expression (3) can be used to establish the link between economic volatility and volatility of earnings. Taking the variance of E_t^* yields:

$$Var(E_t^*) = \beta_1^{*2} Var(E_{t-1}^*) + Var(\varepsilon). \tag{4}$$

The variance of lagged-once earnings is in turn a function of lagged-twice earnings, and so on. Using recursive substitution allows one to reformulate Expression (4) to:

$$Var(E_t^*) = Var(\varepsilon)(1 + \beta_1^{*2} + \beta_1^{*4} + \dots). \tag{5}$$

Expression (5)[1] embodies the intuition that with perfect matching earnings volatility is entirely driven by economic factors. Specifically, the volatility of earnings is increasing in the volatility of economic shocks and in the persistence of these shocks over time.

Poor Matching

As discussed earlier, for the purposes of this model revenue recognition is treated as given, and "poor matching" is defined as a method of expense recognition that deviates from the perfect matching case explored above. Specifically, expenses under poor matching (denoted with no asterisk) are modeled as:

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[1] This expression can be further simplified assuming that one can take the limit of variance of earnings over time: $\lim\{Var(E_t^*)\} = Var(\varepsilon) / (1 - \beta_1^{*2})$.

$$Exp_t = Exp_t^* + \nu_t \tag{6}$$

where $\nu_t = \tau_t - \tau_{t-1}$, and τ is a random variable, which is not correlated with *Rev** and *Exp**. Note that the essence of "bad matching" is modeled by two properties of the ν term in Expression (6). First, ν_t is uncorrelated with *Rev** and *Exp** because by definition there is no causal economic relationship between the mismatched expense and the well-matched revenue and expense.[2] In other words, mismatched expenses are modeled as "noise" in the economic relation of advancing expenses to earn revenues. Second, ν_t has a strong negative first-order autocorrelation, which is clear when one considers that $\nu_t = \tau_t - \tau_{t-1}$ and $\nu_{t-1} = \tau_{t-1} - \tau_{t-2}$. The negative autocorrelation in the noise term captures the fact that accounting is self-correcting, and the mismatching of expenses is eventually resolved and corrected through time.[3] The same property also reflects the fact that in the long run the sum of expenses is expected to converge to the same amount, regardless of the expense recognition method employed. Specifically, in Expression (6) all mismatching reversals are assumed to happen within one period of origination. Thus, the essence of Expression (6) is that recorded expense (Exp_t) differs from the "perfectly matched" expense (Exp_t^*) because of currently mismatched expenses (τ_t) and because of the current correction of previously mismatched expenses (τ_{t-1}). It is also clear from Expression (6) that matching becomes worse when the τ terms are large, and accordingly for the purposes of this model "quality of matching" is defined as the inverse of the variance of τ.

Note also that the representation in Regression (6) is simple but it captures important aspects of matching and accrual accounting in general while allowing flexibility and imposing few restrictive assumptions. For example, consider that τ is a random variable and can be both positive (over-accrual of expenses) and negative (under-accrual of expenses). The τ variable can also have a nonzero mean implying that this framework can accommodate both systematic conservatism and aggressiveness in financial reporting. To illustrate, aggressive recognition of expenses can be modeled by assuming that τ has a negative mean, and the magnitude of the mean can be calibrated to reflect the degree of aggressiveness in reported numbers. Another example is the assumed horizon of mismatching reversals. The representation in Expression (6) assumes that all mismatching of expenses is resolved within one period. However, this is just a convenience and simplification assumption. The analysis that follows and the conclusions remain virtually the same if the reversals happen over longer horizons.

The Effects of Poor Matching

Using the definitions of perfect and poor matching, we turn to establishing the effects of poor matching. More formally, the effects are given by a series of four observations:

Observation 1: Poor matching decreases the contemporaneous correlation between revenues and expenses.

A formal proof is provided in the Appendix. Intuitively, this observation results from the fact that poor matching "scatters" some of the perfectly matched expenses across periods and, therefore, the contemporaneous correlation between observed revenues and expenses is lower than the underlying economic correlation of advancing expenses to produce revenues. Consistent with this intuitive appeal, there is at least one existing study that has used the contemporaneous correlation between revenues and expenses as a measure of matching quality (Sivakumar and Waymire 2003).

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[2] Note that opportunistic managerial smoothing can create situations where the mismatched expenses are correlated with properly matched revenues, expenses, and earnings. For example, to keep earnings smooth managers may under-accrue some expenses when revenues are low or other expenses are high. This possibility is not modeled explicitly because our main interest is in how the evolution of accounting rules and the nature of the real economy could result in poor matching. Thus, the assumption that the mismatched expense and the well-matched revenue and expense are not correlated seems reasonable.

[3] For example, underestimating warranty expense is an example of bad matching but the underestimation will be corrected when the actual warranty expenditures prove to be higher than the initial underestimated expense.

Observation 2: Poor matching increases the volatility of earnings.

Using Expression (6) and the fact that revenues remain the same under both matching alternatives allows one to derive earnings under bad matching as:

$$E_t = Rev_t^* - Exp_t^* - \nu_t = E_t^* - \tau_t + \tau_{t-1}. \qquad (7)$$

Taking the variance of earnings yields:

$$Var(E_t) = Var(E_t^*) + 2Var(\tau) = Var(\varepsilon)(1 + \beta_1^{*2} + \beta_1^{*4} + \dots) + 2Var(\tau). \qquad (8)$$

An inspection of Expression (8) and a comparison with Expression (5) reveal that poor matching increases the volatility of earnings. Intuitively, earnings with poor matching are more volatile because mismatched expenses acts as noise unrelated to the economic process of generating earnings, and thus mismatched expenses add an extra layer of volatility over and above the unavoidable economics-driven volatility.

Observation 3: Poor matching decreases the persistence of earnings.

Persistence of earnings is the slope coefficient from a regression of current earnings on lagged earnings. Recall that the "true relation" under perfect matching is:

$$E_t^* = \beta_0^* + \beta_1^* E_{t-1}^* + \varepsilon_t.$$

However, under imperfect matching we observe $E_t = E_t^* - \nu_t$ and $E_{t-1} = E_{t-1}^* - \nu_{t-1}$. Substituting, using the fact that $\nu_t = \tau_t - \tau_{t-1}$ and re-arranging obtains:

$$E_t + \nu_t = \beta_0^* + \beta_1^*(E_{t-1} + \nu_{t-1}) + \varepsilon_t;$$

$$E_t = \beta_0^* + \beta_1^* E_{t-1} + (\varepsilon_t + \beta_1^* \upsilon_{t-1} - \nu_t);$$

$$E_t = \beta_0^* + \beta_1^* E_{t-1} + \{\varepsilon_t - \tau_t + (1 + \beta_1^*)\tau_{t-1} - \beta_1^* \tau_{t-2}\}. \qquad (9)$$

Note that both the third and the fourth term in the error term in Expression (9) are negatively correlated with the regressor E_{t-1} (since $E_{t-1} = E_{t-1}^* - \tau_{t-1} + \tau_{t-2}$), and denote the error term as $\lambda = \varepsilon - \tau_t + (1 + \beta_1^*)\tau_{t-1} - \beta_1^* \tau_{t-2}$. Since the error term is correlated with the independent variable, OLS estimation produces a biased and inconsistent estimate of β_1^*:

$$\beta_1^{OLS} = \beta_1^* + \frac{\sum E'_{t-1}\lambda'}{\sum E'^{2}_{t-1}}. \qquad (10)$$

An inspection of Expression (10) allows one to conclude that poor matching decreases the slope coefficient β_1 since the summation term in the numerator is negative and the negative autocorrelation in the τ terms makes this effect stronger.

Corollary to Observation 3: Poor matching induces a negative autocorrelation in earnings changes.

Intuitively, this happens because poor matching introduces noise in the economic relation between revenues and expenses. Since noise has strong negative autocorrelation in changes (pure white noise has an autocorrelation in changes of −0.50), introducing noise means inducing a negative autocorrelation in earnings changes. The formal proof is in the Appendix. This observation is presented as a corollary because the decreasing persistence of earnings and the increasing negative autocorrelation in earnings changes are really just two sides of the same effect, and one implies the other in our stylized model. We keep them separate to emphasize the point that low persistence in earnings implies high negative autocorrelation in earnings changes, which means that earnings innovations are largely devoid of new information with respect to future earnings realizations.

Observation 4: The effect of poor matching are alleviated over longer-time horizons.

The formal proof is in the Appendix. Intuitively, this happens because accounting is self-correcting; therefore, by their nature mismatching errors resolve over longer horizons. For example, assume that there is a substantial amount of mismatching at the level of one-year earnings and that all mismatching resolves within one year. Then, a five-year specification will have relatively much less mismatching because all mismatching errors in the middle three of the five years are resolved and only some of the mismatching errors in the first and the last years remain.

Discussion of the Theory and its Relation to Existing Research

The topic of matching has a long history in accounting, and discussions of matching and its importance can be found in a variety of accounting texts, including the classic Paton and Littleton (1940) monograph (hailed as the "Accounting Book of the Century") and virtually all financial accounting textbooks (e.g., Revsine et al. 2005, 41, 46, 51–53). For example, Paton and Littleton (1940) refer to matching as "the principal concern" and "the fundamental problem" of accounting. However, there has been little research effort aimed at matching during the last 10 to 20 years. Perhaps one reason for this lack of research is that in earlier years the dominant paradigm of market efficiency implied that the market fully sees through the accounting conventions and complications of measuring firm performance. It is only recently that there has been a renewed interest into "fundamental analysis," which is the study of whether and how our knowledge of accounting yields superior insights into firm performance and security valuation, e.g., Fairfield et al. (1996), Sloan (1996), Piotroski (2000), Nissim and Penman (2001), and many others; Dechow and Schrand (2004) provide a useful overview of this literature. In the spirit of fundamental analysis, we view the study of matching as a further step into enriching our knowledge about the determination and properties of earnings.

Perhaps another reason for the relative lack of research into matching is the evolution of accounting standards. Early standards recognized the importance of matching on both the conceptual and practical level, e.g., APB No. 11 used matching as the guiding principle for accounting for income taxes. However, during the last two to three decades the Financial Accounting Standards Board (FASB) has adopted an increasingly balance-sheet-based perspective, where the determination of income is viewed more as resulting from revisions of asset and liability values rather than as the residual from revenues and matched expenses (Storey and Storey 1998). Recent examples of a predominantly balance sheet perspective include the accounting for goodwill and other intangible assets in SFAS No. 142 and the accounting for impairment and disposal of long-term assets in SFAS No. 144.

There are three strands of recent research that are close to the spirit or the measures of our study. The first strand comprises Su (2005) and the related studies of Lane and Willet (1999) and Gibbins and Willet (1997). The common thread in these studies is the idea that proper matching of revenues and expenses has a smoothing effect on earnings, which is beneficial in the sense that it allows for better estimation of long-run economic profitability. Our study shares this core intuition about matching but is much different in terms of approach, aims, and empirical analysis. Su (2005) uses a technique called statistical activity cost theory to develop its ideas and relies on computer simulations for its empirical results. We use the language and models of capital market research and rely on actual financial data to draw our conclusions.

Second, a number of existing studies use measures of quality of earnings, which are similar to ours. For example, Francis et al. (2004) use measures of earnings persistence and earnings smoothness (the reverse of volatility), which closely correspond to our constructs; in addition, they provide a number of references to other research that uses comparable variables. Thus, our contribution is not so much in advancing the use of these variables but at providing the unifying matching framework for their use, which suggests a different perspective and different ways to utilize them.

A third strand of related research investigates the matching of cash flows and accruals. For example, Dechow (1994) shows that an important function of accruals is to resolve timing and mismatching problems in the underlying cash flows, e.g., the accrual for inventory purchase recognizes that cash spent on inventory does not necessarily lead to loss of firm value. Dechow and Dichev (2002) and Richardson et al. (2005) argue that accruals alter the timing of the recognition of cash flows into earnings, increasing earnings timeliness and persistence but at the cost of earnings including the noise of unavoidable errors of estimation. Our investigation can be viewed as further development of these themes in the context of matching of revenues and expenses. Later in the paper, we provide some results on the interplay between the matching of revenues and expenses and the matching of cash flows and accruals.

III. EMPIRICAL TESTS

Our empirical tests concentrate on time-series specifications because the evolution of standard-setting and the real economy provide strong reasons to believe that matching has become worse over time. As discussed above, in the late 1970s and early 1980s, the FASB started a deliberate and decisive move away from matching and toward embracing a more balance-sheet-based accounting. We aim to document the effects of this evolution and discuss its implications with regard to the needs of various users of financial statements. We believe that these results are also helpful as a guide to the future, as the FASB and the IASB seem intent on further expanding and solidifying the balance-sheet-based model.

Another factor that likely affects the temporal characteristics of revenues, expenses, and earnings is changes in the real economy. We believe that these changes also work in the direction of finding more poor matching over time, e.g., the general trend of more fixed versus variable costs over time, the rise of R&D-type activities and more complicated production, marketing, and financing arrangements. For the purposes of this paper, we are more interested in documenting the aggregate results from the confluence of these two factors rather than differentiating their relative roles and, therefore, our main results do not control for the effects of one versus the other. Specifically, we test whether discernible patterns in the properties of revenues, expenses, and especially earnings over time is consistent with the theoretical predictions above. Additional tests later provide some evidence on the differential roles of these two sources in poor matching.

Our focus on time-series tests is a primary determinant in the choice of our sample. A substantial problem with time-series tests, especially over long horizons, is preserving comparability while faced with continual and large fluctuations in available firms due to the addition of new firms, various forms of failure and exit, and the constantly changing relative importance of existing firms. An added problem is uneven firm coverage over time, where existing databases like Compustat have much wider and more complete coverage of firms in later years, which can introduce systematic biases in reported results, e.g., Klein and Marquardt (2006). Our pragmatic solution to these problems is at every point in time in our sample to use the top 1,000 firms in terms of total assets. This definition of the sample ensures that early coverage is similar to late coverage (the largest, most prominent firms), while it reflects the changing nature and prominence of firms as the economy evolves. Later, we present robustness tests which specifically consider and control for the changing sample composition over time.

Table 1 summarizes the remaining specifics of our sample selection. The initial sample is obtained from the Compustat annual industrial database, where the sample is restricted to firm-years with complete data for assets (Compustat item 6), revenues (item 12), and earnings before extraordinary items (item 18), and preceding 9 years of revenues and earnings. Since we use both one-year and two-year (the longer horizon) definitions for our variables, the sample selection criteria are mostly based on two-year availability because the two-year definitions are more restrictive. In order to limit the sample to economically substantial firms, we pick the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available. As volatility in two-year earnings takes up to 10 years of data to calculate, the final two-year sample starts in 1967 and ends in 2003. To avoid the influence of extreme observations, we delete the top and bottom 1 percent of all two-year variables, including two-year earnings, prior two-year earnings, volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses. The final two-year sample consists of 17,788 firm-year observations from 1967 to 2003. To ensure proper comparisons across samples, only firms in the two-year sample are included in the one-year sample. To

TABLE 1

Derivation of the Sample

Compustat firm-years with available total assets, revenues, and earnings	297,692
Firm-years with available deflated revenues, earnings, and expenses	272,625
Firm-years with available non-overlapping two-year revenues, two-year earnings, and two-year expenses	134,704
Firm-years with available data on volatility in two-year earnings, two-year revenues, and two-year expenses	45,117
Firm-year observations in the two-year sample where firms are among the largest 1,000 firm for each odd-year from 1967 to 2003	18,564
Firm-years remaining in the two-year sample after truncating the top and bottom 1 percent of all two-year variables	17,788
Firm-years in the final two-year sample	**17,788**
Firm-year observations in the one-year sample where firms are in the two-year sample	37,116
Firm-years remaining after truncating the top and bottom 1 percent of all one-year variables	34,785
Firm-years in the final one-year sample	**34,785**

avoid the influence of extreme observations, we delete the top and bottom 1 percent of all one-year variables, and the final one-year sample consists of 34,785 observations from 1967 to 2003.

Descriptive statistics for the final one-year and two-year samples are presented in Table 2. Variable definitions are as follows. Expenses are estimated by taking the difference between revenues and earnings before extraordinary items. Earnings, revenues, and expenses are deflated by average assets. Note that deflating by assets largely resolves scale issues, which can be problematic for long-horizon time-series studies. Revenue, expense, and earnings volatility are

TABLE 2

Descriptive Statistics

Variable	**n**	**Mean**	**Std. Dev.**	**Median**
Earnings	34,785	0.046	0.052	0.044
Revenues	34,785	0.988	0.838	0.850
Expenses	34,785	0.942	0.826	0.802
Vol (Earnings)	34,785	0.018	0.020	0.012
Vol (Revenues)	34,785	0.097	0.095	0.069
Vol (Expenses)	34,785	0.091	0.091	0.063
Corr (Revenues, Expenses)	34,785	0.944	0.162	0.993
Two-Year Earnings	17,788	0.047	0.047	0.045
Two-Year Revenues	17,788	0.998	0.840	0.861
Two-Year Expenses	17,788	0.951	0.828	0.813
Vol (Two-Year Earnings)	17,788	0.018	0.017	0.013
Vol (Two-Year Revenues)	17,788	0.126	0.120	0.092
Vol (Two-Year Expenses)	17,788	0.119	0.116	0.084
Corr (Two-Year Revenues, Two-Year Expenses)	17,788	0.965	0.113	0.995
Persistence in Earnings	37	0.782	0.107	0.786
Autocorrelation in Earnings Change	37	−0.123	0.166	−0.110
Persistence in Two-Year Earnings	19	0.751	0.085	0.747
Autocorrelation in Two-Year Earnings Change	19	−0.133	−0.131	0.132

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). ***Revenues*** is net revenues (Compustat item 12) deflated by average assets. ***Expenses*** is the difference between ***Revenues*** and ***Earnings***. ***Vol (Earnings)*** is earnings volatility, which is calculated by taking the standard deviation of the deflated earnings for the most recent five years. ***Vol (Revenues)*** is revenues volatility, which is calculated by taking the standard deviation of the deflated revenues for the most recent five years. ***Vol (Expenses)*** is expenses volatility, which is calculated by taking the standard deviation of the deflated expenses for the most recent five years. ***Corr (Revenues, Expenses)*** is the correlation between revenues and expenses, which is calculated as the correlation between the deflated revenues and the deflated expenses for the most recent five years. ***Two-Year Earnings*** are calculated as the average of deflated earnings for the current and previous periods. ***Two-Year Revenues*** and ***Expenses*** are calculated analogously. ***Vol (Two-Year Earnings)*** is volatility in two-year earnings, which is calculated by taking the standard deviation of two-year earnings for the most recent five non-overlapping two-year periods. ***Vol (Two-Year Revenues)*** and ***Vol (Two-Year Expenses)*** is calculated analogously. ***Corr (Two-Year Revenues, Two-Year Expenses)*** is calculated analogously for the most recent five non-overlapping two-year periods. For each sample year, ***Persistence in Earnings*** is the slope coefficients from the regression of current deflated earnings on the previous period earnings on a cross-section basis. ***Autocorrelation in Earnings Change*** is the cross-sectional correlation between current earnings change and past earnings change. For every other sample year, ***Persistence in Two-Year Earnings*** is the slope coefficients from the regression of current two-year earnings on past two-year earnings on a cross-section basis. ***Autocorrelation in Two-Year Earnings Change*** is the cross-sectional correlation between current two-year earnings change and past two-year earnings change.

calculated by taking the standard deviation of the deflated variable for the most recent five years. The correlation between revenues and expenses is calculated as the correlation between the deflated revenues and the deflated expenses for the most recent five years. Two-year earnings, revenues, and expenses are calculated as the corresponding averages for the current- and last-year periods. Accordingly, volatility in two-year earnings, revenues, and expenses is calculated by taking the standard deviation of the corresponding two-year variable for the most recent five non-overlapping two-year periods. The correlation between two-year revenues and two-year expenses is calculated as the correlation between two-year revenues and two-year expenses for the most recent five non-overlapping two-year periods. For each year, earnings persistence is the slope coefficients from the regression of current deflated earnings on the previous period earnings on a cross-sectional basis. Autocorrelation in earnings changes is the cross-sectional correlation between current earnings changes and past earnings changes. Earnings persistence and autocorrelation in two-year earnings changes are defined analogously. The descriptive statistics in Table 2 are generally in line with much existing research on comparable samples and variables. For example, the sample-wide persistence of earnings is 0.78, indicating fairly high persistence coupled with mean reversion.

We start with an analysis of the trends in the relation between revenues and expenses. Since matching has likely become worse over time, based on Observation 1 we expect that the correlation between revenues and contemporaneous expenses has become weaker over the last 40 years. We also present results for the correlations between revenues and non-contemporaneous expenses based on the conjecture that poor matching scatters expenses to periods away from their associated revenues, resulting in an increased correlation between revenues and non-contemporaneous expenses. We test these predictions by examining the temporal behavior of coefficients in a regression of revenues on one-year-back, present, and one-year-forward expenses. The advantage of the multivariate specification is that it controls for the strong autocorrelation in expenses, which is especially important in examining the relation between revenues and non-contemporaneous expenses. In addition, since past, present, and future expenses have about the same underlying variation, a comparison of the coefficients produces the same results as a comparison of the (incremental) correlations between revenues and expenses.

The yearly results for the expense coefficients are presented in Table 3, while results smoothed over five years are presented in Figure 1. An examination of Figure 1 reveals clear evidence of the hypothesized relations. The coefficient on present expenses starts at a high and consistent value of slightly over 1 but becomes much lower and more erratic in the second half of the examined period. In terms of numbers, Table 3 reveals that the average is 1.03 for the first half of the sample period but decreases to an average of 0.88 in the second half, for a difference of −0.15, which is highly statistically significant. In contrast, the coefficients for past and future expenses hover at or below zero in the beginning, but later visibly increase, in a volatile but unmistakable manner. The increase is especially pronounced for past expenses, where the mean coefficient is 0.007 in the first half of the sample, increasing to 0.101 in the second half, for an increase of 0.094, which is highly statistically significant. The pronounced effect for past expenses is consistent with evidence of increased conservatism over time (Givoly and Hayn 2000), which implies that expenses are increasingly booked before the associated revenue. The increase for future expenses is more modest at 0.055 but is also highly statistically significant. To our knowledge, the relation between current revenues and future expenses has not been documented before. Using the intuition from our model, the possible driving forces for this relation are various underprovisionings of current expenses, which trigger a catch-up effect later. For example, understating necessary depreciation expense today maps into an asset impairment later or underprovisioning for current warranty or bad debt expense (driven by current revenue) triggers catch-up provisions in subsequent periods.

TABLE 3
Regression of *Revenues* on Previous, Current, and Future *Expenses*

Model: $Revenues_t = \alpha + \beta_1{}^{*}Expenses_{t-1} + \beta_2{}^{*}Expenses_t + \beta_3{}^{*}Expenses_{t+1}$

Year	Coefficient on Past *Expenses* (β_1)	Coefficient on Current *Expenses* (β_2)	Coefficient on Future *Expenses* (β_3)
1967	−0.010	1.029	−0.013
1968	−0.014	1.044	−0.015
1969	−0.004	1.030	−0.012
1970	0.002	1.042	−0.033
1971	0.026	1.003	−0.016
1972	0.010	1.089	−0.077
1973	0.063	0.939	0.020
1974	−0.053	1.106	−0.038
1975	0.023	1.061	−0.066
1976	0.028	0.991	0.005
1977	−0.001	1.015	0.007
1978	−0.007	1.053	−0.022
1979	−0.007	1.027	0.006
1980	−0.021	1.070	−0.028
1981	0.063	0.965	−0.010
1982	−0.017	1.054	−0.024
1983	−0.016	1.087	−0.056
1984	0.051	0.972	0.003
1985	0.016	1.013	−0.013
1986	0.039	0.937	0.038
1987	0.145	0.762	0.111
1988	−0.013	1.032	0.007
1989	0.066	1.003	−0.053
1990	0.101	0.932	−0.018
1991	0.176	0.802	0.028
1992	0.117	0.871	0.029
1993	0.168	0.691	0.152
1994	0.033	0.986	0.006
1995	0.029	0.979	0.018
1996	0.020	1.000	0.006
1997	0.093	0.894	0.038
1998	0.032	0.977	0.016
1999	0.081	0.952	−0.005
2000	0.042	1.015	−0.037

(continued on next page)

TABLE 3 (continued)

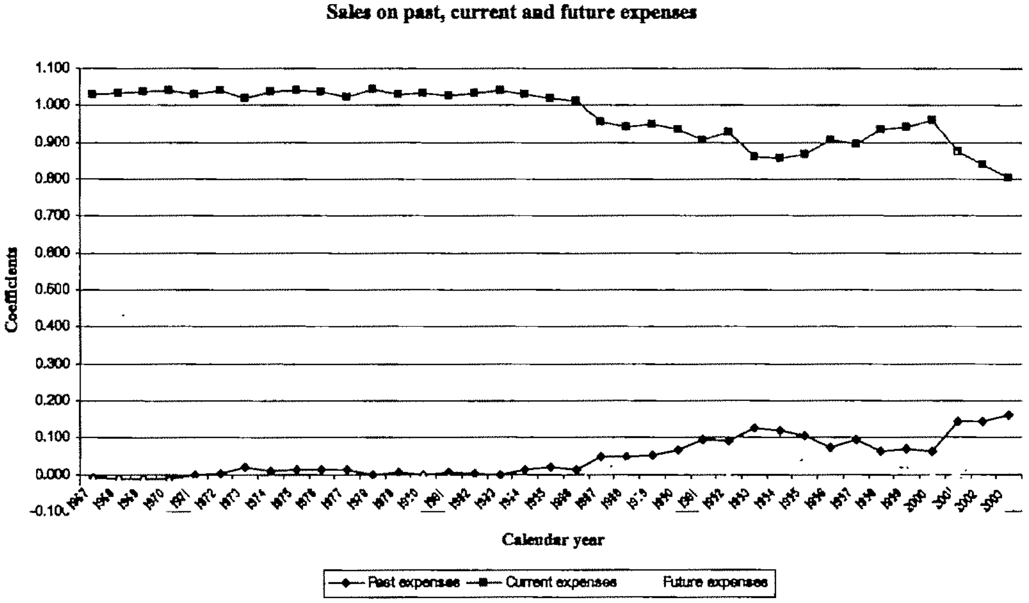
Year	Coefficient on Past *Expenses* (β_1)	Coefficient on Current *Expenses* (β_2)	Coefficient on Future *Expenses* (β_3)
2001	0.464	0.533	−0.012
2002	0.092	0.715	0.204
2003	0.132	0.797	0.091
Mean 1967 to 1985	0.007	1.031	−0.020
Mean 1986 to 2003	0.101	0.882	0.034
Difference	0.094	−0.149	0.055
p-value on difference	<0.001	<0.001	0.002

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Revenues*$_t$ is net revenues (Compustat item 12) deflated by average assets for the current period. *Expenses*$_t$ is the difference between *Revenues* and *Earnings* for the current period. *Expenses*$_{t-1}$ is the difference between *Revenues* and *Earnings* for the previous period. *Expenses*$_{t+1}$ is the difference between *Revenues* and *Earnings* for the next period. The regression is run on a cross-sectional basis each year. The slope coefficients on past, current, and future expenses are reported in the table. p-value on the difference is obtained from a two-tailed t-test.

Next, we turn to documenting the effect of poor matching on earnings volatility. From Observation 2 and the conjecture that matching has become worse over time, we expect to see that earnings volatility has increased over time. To reflect the fact that earnings volatility arises from the volatility of the underlying fundamentals in addition to poor matching, we also include accompanying results for revenues and expenses. Specifically, since earnings is equal to revenues minus expenses, the volatility of earnings is equal to the volatility of revenues plus volatility of expenses, adjusted for the correlation between revenues and expenses, so we include the data for all these variables. The value of this analysis is that (as discussed above) the correlation between revenues and expenses reflects the quality of matching, while the volatilities of revenues and expenses can be thought of as capturing the volatility of the underlying business fundamentals, and thus one can assess whether the increase in earnings volatility is due to increase in the volatility of fundamentals.

Table 4, Panel A contains the results for the one-year specification, providing mean values for each year in the sample and t-tests for differences in means between the first and second half of the sample period. These results are also graphically depicted in Figure 2, Panel A, where all values are normalized to a beginning value of 100 and smoothed over five years to enhance comparability. An inspection of Figure 2 and Table 4 reveals that earnings volatility has substantially increased over the last four decades, and that this increase is highly statistically significant. The inspection also reveals that the increase in earnings volatility cannot be attributed to increase in the volatility of fundamentals because there is no increase (and an actual slight decrease) in the volatilities of revenues and expenses. In contrast, there is a material and highly statistically

FIGURE 1
Coefficients in Regression of Revenues on Past, Current, and Future Expenses over 1967–2003



This figure plots the five-year moving average (the average for the current and previous four years) of slope coefficients of regression of current revenues on past, current, and future expenses from year 1967 to year 2003. *Earnings* is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Revenues* is net revenues (Compustat item 12) deflated by average assets. $Revenues_t$ is net revenues (Compustat item 12) deflated by average assets for the current period. $Expenses_t$ is the difference between *Revenues* and *Earnings* for the current period. $Expenses_{t-1}$ is the difference between *Revenues* and *Earnings* for the previous period. $Expenses_{t+1}$ is the difference between *Revenues* and *Earnings* for the next period. For each sample year, the regression of current revenues on past, current, and future expenses is run on a cross-sectional basis.

significant decrease in the correlation of revenues and expenses over time. The joint consideration of these two results implies that the trend of increasing earnings volatility over time is entirely due to poor matching.[4]

Note that the results for increasing volatility of earnings are not new. Givoly and Hayn (2000) find a great temporal increase in earnings volatility, interpreting it as due to the increase in reporting conservatism over time. While our interpretation is based on temporal deterioration of the matching process, it is useful to point out that these two perspectives share much of the same underlying process. In essence, conservatism can be viewed as a form of "poor matching," where

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[4] Note that whether one takes increased volatility as a sign of higher or lower earnings quality partly depends on the setting and the goals of the investigation. Leuz et al. (2003) posit that lower earnings volatility indicates excessive smoothing, and therefore lower earnings quality. However, the Leuz et al. (2003) perspective seems less relevant in our aggregate time-series setting. The reason is that a number of studies have documented a decline in the quality of earnings over time (e.g., Collins et al. 1997), while other studies have documented a steady increase in the volatility of earnings (e.g., Givoly and Hayn 2000); the combination of these two findings is at odds with the Leuz et al. (2003) story.

TABLE 4
Volatility of Earnings and Its Components over Time

Panel A: Volatility over Time for the One-Year Sample

Year	n	*Vol (Earnings)*	*Vol (Revenues)*	*Vol (Expenses)*	*Corr (Revenues, Expenses)*
1967	779	0.014	0.099	0.092	0.972
1968	811	0.014	0.107	0.100	0.977
1969	810	0.014	0.108	0.101	0.978
1970	883	0.014	0.105	0.097	0.973
1971	884	0.015	0.110	0.101	0.966
1972	957	0.014	0.115	0.107	0.968
1973	955	0.013	0.105	0.098	0.969
1974	957	0.011	0.089	0.082	0.973
1975	955	0.014	0.106	0.098	0.979
1976	957	0.014	0.102	0.095	0.981
1977	957	0.014	0.100	0.094	0.980
1978	959	0.014	0.094	0.088	0.971
1979	959	0.014	0.090	0.084	0.972
1980	958	0.013	0.089	0.083	0.978
1981	956	0.012	0.089	0.084	0.976
1982	954	0.013	0.092	0.087	0.974
1983	955	0.017	0.102	0.094	0.970
1984	953	0.019	0.107	0.097	0.970
1985	957	0.018	0.104	0.095	0.968
1986	954	0.017	0.097	0.090	0.954
1987	958	0.018	0.106	0.100	0.951
1988	955	0.019	0.111	0.105	0.943
1989	955	0.020	0.113	0.107	0.933
1990	961	0.020	0.105	0.099	0.924
1991	958	0.020	0.094	0.089	0.909
1992	961	0.021	0.089	0.083	0.907
1993	959	0.020	0.080	0.075	0.905
1994	956	0.020	0.073	0.069	0.896
1995	956	0.020	0.069	0.066	0.892
1996	954	0.020	0.073	0.070	0.895
1997	957	0.020	0.077	0.073	0.901
1998	957	0.021	0.084	0.081	0.904
1999	960	0.021	0.092	0.089	0.920
2000	958	0.023	0.098	0.093	0.915
2001	959	0.026	0.102	0.097	0.916
2002	958	0.029	0.107	0.101	0.890

(continued on next page)

TABLE 4 (continued)

Year	**n**	***Vol (Earnings)***	***Vol (Revenues)***	***Vol (Expenses)***	***Corr (Revenues, Expenses)***
2003	953	0.030	0.107	0.101	0.898
Mean 1967 to 1985		0.014	0.101	0.094	0.973
Mean 1986 to 2003		0.021	0.093	0.088	0.914
Difference		0.007	−0.008	−0.005	−0.059
p-value on Difference		<0.001	0.057	0.140	<0.001

Panel B: Volatility over Time for the Two-Year Sample

Year	**n**	***Vol (Two-Year Earnings)***	***Vol (Two-Year Revenues)***	***Vol (Two-Year Expenses)***	***Corr (Two-Year Revenues, Two-Year Expenses)***
1967	776	0.017	0.134	0.126	0.979
1969	812	0.016	0.130	0.123	0.975
1971	883	0.015	0.126	0.119	0.976
1973	961	0.017	0.142	0.134	0.972
1975	956	0.014	0.124	0.117	0.977
1977	958	0.015	0.126	0.119	0.984
1979	959	0.014	0.121	0.114	0.983
1981	957	0.014	0.119	0.112	0.985
1983	957	0.014	0.123	0.117	0.980
1985	957	0.017	0.127	0.119	0.982
1987	956	0.017	0.122	0.114	0.978
1989	957	0.020	0.140	0.130	0.974
1991	958	0.020	0.143	0.135	0.966
1993	956	0.022	0.137	0.129	0.951
1995	957	0.022	0.128	0.121	0.943
1997	956	0.021	0.109	0.102	0.939
1999	957	0.023	0.106	0.100	0.930
2001	958	0.024	0.114	0.109	0.937
2003	957	0.026	0.129	0.121	0.933
Mean 1967 to 1985		0.015	0.127	0.120	0.979
Mean 1987 to 2003		0.022	0.125	0.118	0.950
Difference		0.006	−0.002	−0.002	−0.029
p-value on difference		<0.001	0.730	0.674	<0.001

(continued on next page)

TABLE 4 (continued)

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Revenues* is net revenues (Compustat item 12) deflated by average assets. *Expenses* is the difference between *Revenues* and *Earnings*. *Vol (Earnings)* is earnings volatility, which is calculated by taking the standard deviation of the deflated earnings for the most recent five years. *Vol (Revenues)* and *Vol (Expenses)* are defined analogously. *Corr (Revenues, Expenses)* is the correlation between revenues and expenses, which is calculated as the correlation between the deflated revenues and the deflated expenses for the most recent five years. *Two-Year Earnings* are calculated as the average of deflated earnings for the current and previous periods. *Two-Year Revenues* and *Expenses* are defined analogously. *Vol (Two-Year Earnings)* is calculated by taking the standard deviation of two-year earnings for the most recent five non-overlapping two-year periods. *Vol (Two-Year Revenues)* and *Vol (Two-Year Expenses)* are defined analogously. *Corr (Two-Year Revenues, Two-Year Expenses)* is the correlation between two-year revenues and two-year expenses, which is calculated as the correlation between two-year revenues and two-year expenses for the most recent five non-overlapping two-year periods. p-value on the difference is obtained from a two-tailed t-test.

the expenses precede the associated revenues. Thus, these two interpretations are often just two different lenses through which to view the same effects, and whether one adopts one versus the other partly depends on the goals and the particular needs of the user. We adopt the matching lens because it has not been explored before and because it allows us to offer insights unavailable from the existing conservatism perspective. For example, the matching perspective allows a clear explanation for the substantial increase in the correlation between current revenues and *future* expenses, which is the opposite of what increasing conservatism would predict.

As an aside, note that the results in Figure 1 and Table 3 essentially offer an interesting new measure of conservatism. They suggest that the extent to which "expenses lead revenues" can be used as a natural and practical measure of conservatism. This measure looks natural because recognizing expenses before their associated revenues seems to capture the very essence of conservatism, at least in its income statement implications. It is also practical because the specification in Table 3 is easy to replicate and adapt to a host of other settings, where the magnitude of the coefficient on "expenses lead revenues" is the quantitative measure of degree of conservatism. This new measure could be useful to other research, given the recent surge of interest in this area, and the limitation of some existing measures of conservatism, especially market-based specifications, e.g., Dietrich et al. (2007).

Table 4, Panel B presents the corresponding volatility results for the two-year specifications of all variables. From Observation 4 and the intuition that the effects of poor matching resolve over time, we expect that the temporal patterns of increased volatility of earnings and decreased contemporaneous correlation between revenues and expenses are less-pronounced for the two-year specifications. This expectation is partially borne out in the data. The decline in the correlation between revenues and expenses in Panel B is only half of that in Panel A, and this difference is significant at the 0.001 level based on bootstrap tests as in Noreen (1989).[5] Thus, there is clear evidence that the decline in the quality of matching is less pronounced for longer-horizon definitions. The increase in volatility of earnings is also smaller for Panel B as compared to that in Panel A but in this case the difference is minimal (0.006 versus 0.007) and not statistically significant (bootstrap p-value = 0.240). The most likely reason for the minimal difference in volatility is that

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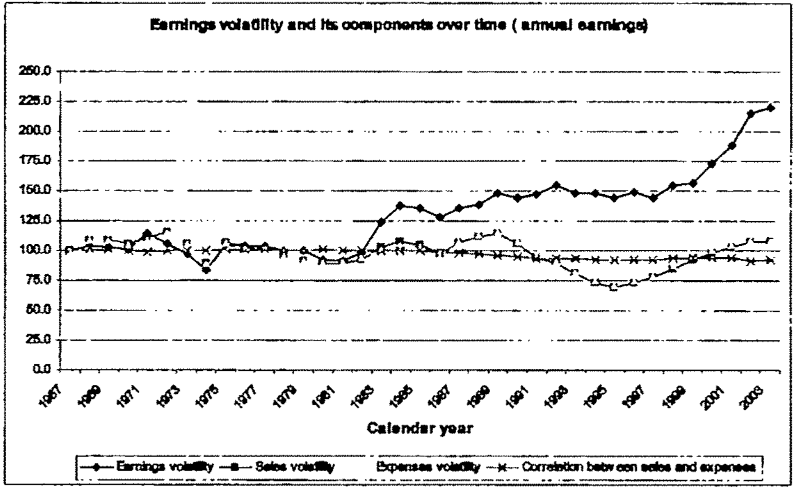
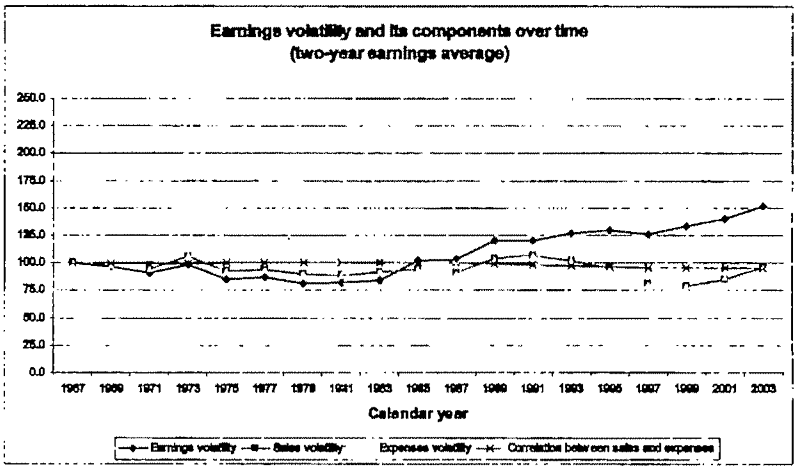
[5] The bootstrap tests are only a minor part of our analysis, so a complete exposition and explanation of these tests is omitted here, see "approximate randomization" in Noreen (1989) for more detail. The basic idea is that by randomly shuffling the time vectors of observed correlations, one can obtain the distribution of differences under the null hypothesis that the difference is purely due to chance, and then the comparison of the actual observed value of the difference versus the simulated null distribution provides the test of statistical significance.

FIGURE 2
Earnings Volatility and Its Components over Time

Panel A: Volatility for the One-Year Sample



Panel B: Volatility for the Two-Year Sample



Panel A plots the normalized earnings volatility, revenues volatility, expenses volatility, and the correlation between revenues and expenses. Panel B plots the normalized two-year earnings volatility, two-year revenues volatility, two-year expenses volatility, and the correlation between two-year revenues and two-year expenses.

The sample starts from 1967 and ends on 2003. In order to limit the sample to economically substantial firms, we pick up the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available.

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Revenues* is net revenues (Compustat item 12) deflated by average assets. *Expenses* is the difference between *Revenues* and *Earnings*. *Vol* (*Earnings*) is earnings volatility, which is calculated by taking the standard deviation of the deflated earnings for the most recent five years. *Vol* (*Revenues*) is revenues volatility, which is calculated by taking the standard deviation of the deflated revenues for the most recent five years. *Vol* (*Expenses*) is expenses volatility, which is calculated by taking the standard deviation of the deflated expenses for the most recent five years. *Corr* (*Revenues, Expenses*) is the correlation between revenues and expenses, which is calculated as the correlation between the deflated revenues and the deflated expenses for the most recent five years. *Two-Year Earnings* are calculated as the average of deflated earnings for the current and previous periods. *Two-Year Revenues* and *Expenses* are calculated analogously. *Vol* (*Two-Year Earnings*) is volatility in two-year earnings, which is calculated by taking the standard deviation of two-year earnings for the most recent five non-overlapping two-year periods. *Vol* (*Two-Year Earnings*) and *Vol* (*Two-Year Expenses*) is calculated analogously. *Corr* (*Two-Year Revenues, Two-Year Expenses*) is calculated analogously for the most recent five non-overlapping two-year periods. Normalize all volatility and correlation variables by their values for the beginning year of 1967.

our longer horizon is only two years. Thus, the included two-year results should be more properly considered as the lower limit on how the consideration of longer-horizon variables attenuates the effects of poor matching.[6]

The results for persistence of earnings and autocorrelation in earnings changes are presented in Figure 3 and Table 5, including one-year and two-year specifications. Based on Observation 3 and its Corollary, we expect that earnings persistence has declined over time and that the autocorrelation in earnings changes has become more negative. An informal inspection of the smoothed one-year results in Figure 3, Panel A reveals a clear support for this conjecture, with steady downward drift in both variables over time. The numerical results and tests in Panel A in Table 5 also confirm this pattern. The mean earnings persistence over the first half of the sample period is 0.855, while it is only 0.705 over the second half of the sample, for a difference of −0.15, which is highly statistically significant. The mean autocorrelation in earnings changes is only −0.019 in the first half but becomes −0.234 in the second half, for a difference of −0.215, which is highly statistically significant.

We aim to provide some additional feel for the economic importance of these results by using a regression of the yearly earnings persistence and autocorrelation variables on an ordinal time variable, where the calendar year is replaced by its time rank (e.g., the first year 1967 is 0 and the last year 2003 is 36). The regression results in Panel B of Table 5 confirm that earnings persistence is declining over time, with a negative and highly significant coefficient on the time variable. More relevant in terms of economic importance, the regression reveals an adjusted R^2 of 0.54, which suggests that this downward trend is the defining feature of the evolution of earnings persistence over time, accounting for more than half of its time-series variation. The fitted value for earnings persistence for the first year of the sample (1967) is 0.91 and the corresponding number for the last year is 0.65, which reveals an economically substantial decline in earnings persistence over the last four decades.[7] We repeat the same regression analysis for the autocorrelation in earnings changes. The R^2 is 0.39, which suggests that more than a third of the total temporal variation in the autocorrelation variable is due to its secular decline over time. The fitted value for the first year is 0.053 and the one for the last year is −0.299. Thus, while the beginning value is close to zero, by the end of the sample, earnings exhibits economically strong reversal in changes, where nearly a third of the typical change is immediately reversed in the next period.

The two-year results in Figure 3 and Table 5 provide further evidence whether the effects of poor matching are alleviated for longer-horizon variables. An informal inspection and comparison of Figure 3, Panels A and B, indicates that this is likely the case because the pronounced downward drifts in Figure 3, Panel A are attenuated in Figure 3, Panel B. Turning to the numbers in Panel C of Table 5, we find further support for this conjecture. The difference in means between the first half and the second half of the sample is −0.111 for the two-year specification as compared to −0.150 in Panel A, and this attenuation is marginally significant (bootstrap p-value = 0.09). The results are much more emphatic for the autocorrelation in earnings changes, where the time-series difference in means in Panel C is only −0.058 as compared to −0.215 in Panel A, an attenuation which is both statistically and economically significant with a bootstrap p-value of

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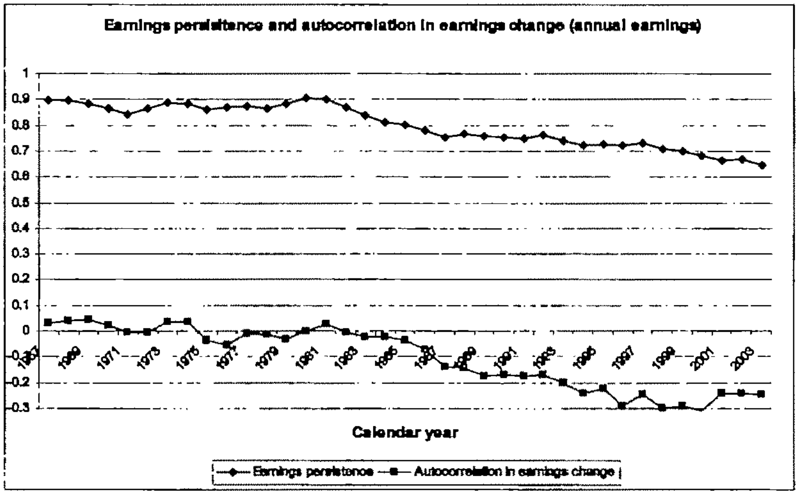
[6] We also investigated a four-year and a quarterly specification to vary the relative horizon of the examined effects but eventually had to abandon these specifications because they led to unacceptable loss in comparability in our setting. The four-year specification and the need for five periods to calculate volatilities leads to losing half of our time-series. For the quarterly specification, the difficulty is in substantial and varying differences in the properties of fourth quarter versus the other quarters' variables.

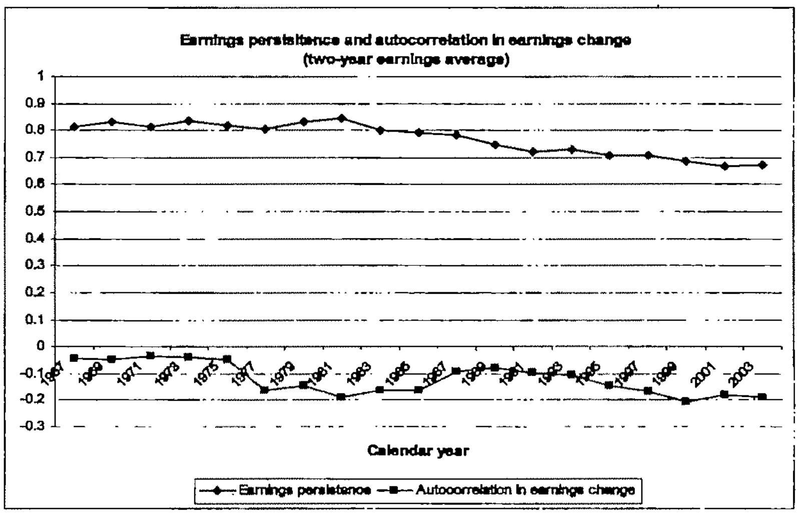
[7] We use the fitted values because, assuming that the fitted line is "correct," the fitted values are a better representative of the "true" relation. In any case, an inspection of the actual values in Panel A of Table 5, reveals pretty much the same impression in terms of magnitude of the decline, if anything, the actual difference between the first and the last year is 0.29, which is larger than the fitted difference of 0.26.

FIGURE 3
Earnings Persistence and Autocorrelation in Earnings Changes over Time

Panel A: Persistence and Autocorrelation for the One-Year Sample



Panel B: Persistence and Autocorrelation in Earnings Changes for the One-Year Sample



Panel A plots the five-year moving average (the average for the current and the previous four years) of persistence in earnings and autocorrelation in earnings change.

Panel B plots the five two-year moving average (the average for the current and the previous four two-year periods) of persistence in two-year earnings and autocorrelation in two-year earnings change.

The sample starts from 1967 and ends on 2003. In order to limit the sample to economically substantial firms, we pick up the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available.

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Revenues* is net revenues (Compustat item 12) deflated by average assets. *Expenses* is the difference between *Revenues* and *Earnings*. For each sample year, *Persistence in Earnings* is the slope coefficients from the regression of current deflated earnings on the previous period earnings on a cross-section basis. *Autocorrelation in Earnings Change* is the cross-sectional correlation between current earnings change and past earnings change. For every other sample year, *Persistence in Two-Year Earnings* is the slope coefficients from the regression of current two-year earnings on past two-year earnings on a cross-section basis. *Autocorrelation in Two-Year Earnings Change* is the cross-sectional correlation between current two-year earnings change and past two-year earnings change.

TABLE 5
Persistence in Earnings and Autocorrelation in Earnings Changes over Time

Panel A: Persistence and Autocorrelation over Time for the One-Year Sample

Year	n	*Persistence in Earnings*	*Autocorrelation in Earnings Change*
1967	970	0.796	−0.089
1968	969	0.874	−0.060
1969	969	0.886	0.078
1970	967	0.832	0.065
1971	969	0.835	−0.025
1972	971	0.911	−0.069
1973	966	0.976	0.141
1974	964	0.855	0.072
1975	962	0.722	−0.298
1976	969	0.891	−0.111
1977	969	0.928	0.155
1978	967	0.936	0.115
1979	968	0.943	−0.023
1980	968	0.840	−0.132
1981	968	0.852	0.023
1982	970	0.780	0.004
1983	968	0.770	0.023
1984	964	0.829	−0.031
1985	965	0.795	−0.188
1986	969	0.718	−0.156
1987	969	0.659	−0.338
1988	966	0.842	0.008
1989	968	0.776	−0.195
1990	969	0.779	−0.180
1991	969	0.688	−0.161
1992	966	0.731	−0.327
1993	966	0.726	−0.137
1994	968	0.693	−0.395
1995	969	0.800	−0.093
1996	969	0.657	−0.499
1997	970	0.772	−0.115
1998	968	0.615	−0.396
1999	964	0.657	−0.343
2000	965	0.708	−0.186
2001	964	0.570	−0.167

(continued on next page)

TABLE 5 (continued)

Year	n	*Persistence in Earnings*	*Autocorrelation in Earnings Change*
2002	967	0.786	−0.106
2003	964	0.509	−0.422
Mean 1967 to 1985		0.855	−0.019
Mean 1986 to 2003		0.705	−0.234
Difference		−0.150	−0.215
P-value on difference		<0.001	<0.001

Panel B: Regression Results for Time Trends in Persistence and Autocorrelation

$$Persistence\ in\ Earnings_t = b_1 + b_2{}^{*}t + \varepsilon$$

$$Autocorrelation\ in\ Earnings\ Changes_t = b_1 + b_2{}^{*}t + \varepsilon$$

where t is an ordinal time variable (1967 = 0 and 2003 = 36).

Regression	b_1 (t-stat)	b_2 (t-stat)	R^2	Fitted Value Year 1967	Fitted Value Year 2003
Persistence of Earnings	0.914 (38.97)	−0.0073 (−6.53)	0.54	0.914	0.651
Autocorrelation in Earnings Changes	0.053 (1.25)	−0.0098 (−4.87)	0.39	0.053	−0.299

Panel C: Persistence and Autocorrelation over Time for the Two-Year Sample

Year	n	*Persistence in Two-Year Earnings*	*Autocorrelation in Two-Year Earnings Change*
1967	968	0.848	0.080
1969	966	0.786	−0.153
1971	967	0.746	0.049
1973	968	0.893	−0.213
1975	968	0.813	−0.006
1977	965	0.787	−0.494
1979	967	0.921	−0.056
1981	966	0.809	−0.182
1983	968	0.666	−0.073
1985	963	0.769	−0.012
1987	968	0.747	−0.129
1989	965	0.753	−0.004

(continued on next page)

TABLE 5 (continued)

Year	n	*Persistence in Two-Year Earnings*	*Autocorrelation in Two-Year Earnings Change*
1991	970	0.671	−0.257
1993	966	0.705	−0.131
1995	968	0.675	−0.207
1997	966	0.731	−0.228
1999	970	0.644	−0.212
2001	965	0.587	−0.123
2003	965	0.722	−0.183
Mean 1967 to 1985		0.804	−0.106
Mean 1987 to 2003		0.693	−0.164
Difference		−0.111	−0.058
p-value on difference		0.002	0.340

Earnings is earnings before extraordinary items (Compustat item 18) deflated by average assets (Item 6). *Two-Year Earnings* are calculated as the average of deflated earnings for the current and previous periods. For each sample year, *Persistence in Earnings* is the slope coefficients from the regression of current deflated earnings on the previous period earnings on a cross-section basis. *Autocorrelation in Earnings Change* is the cross-sectional correlation between current earnings change and past earnings change. For every other sample year, *Persistence in Two-Year Earnings* is the slope coefficients from the regression of current two-year earnings on past two-year earnings on a cross-section basis. *Autocorrelation in Two-Year Earnings Change* is the cross-sectional correlation between current two-year earnings change and past two-year earnings change. p-value on the difference is obtained from a two-tailed t-test.

0.001. As already discussed, this pattern of attenuation in two-year results should be probably thought of as a lower limit on how the effects of poor matching are attenuated in longer-horizon specifications of variables.

IV. ROBUSTNESS CHECKS AND ADDITIONAL RESULTS

The main results above provide strong evidence that there have been substantial changes in the properties of accounting earnings over the last 40 years. In this section we provide a number of additional tests that serve as robustness checks and also offer evidence about the relative importance of the economic and accounting causes of this phenomenon.

First, we investigate the effect of one-time items and losses. A variety of one-time items have proliferated in frequency and magnitude over the last 30 years (e.g., Collins et al. 1997; Bradshaw and Sloan 2002), and this trend could provide an alternative explanation for some (but not all) of our results. For example, the proliferation of one-time items can potentially account for the decrease in the correlation between contemporaneous revenues and expenses, the increase in earnings volatility, and the decrease in earnings persistence (although it cannot explain the increased correlation between revenues and non-contemporaneous expenses and the lack of meaningful increase in the volatility of expenses). However, the analysis of one-time items has to be done with caution because the proliferation of one-time items is itself a prime manifestation of the deterioration of matching over time. Most one-time items arise because of various asset revaluations or "unusual" charges, which indicate an economic—or rules-imposed lack of relation between revenues and expenses. Thus, a simple elimination of one-time items is objectionable because it is akin to "throwing out the baby with the bathwater" effect.

We address this problem in a pragmatic manner, where we re-run our main tests with expenses and earnings excluding one-time items while we emphasize that these results should be more properly viewed as an illustration for how much of the effects are concentrated in one-time items rather than as whether one-time items "explain" the main results in the paper. Operationally, one-time item is defined as Compustat item 17. We find that the exclusion of one-time items leads to qualitatively similar results. Specifically, the elimination of special items reduces the magnitude of the temporal differences to about half of their original values but these differences remain economically and statistically significant. For example, in the regression of revenues on expenses, the mean slope coefficient on contemporary expenses declines from 1.067 to 0.986 over the two subsample periods, and the difference is highly significant (p-value = 0.0007). Mean earnings volatility increases from 0.014 to 0.019 over the two subsample periods, and this difference is also statistically significant (p-value = 0.001). The rest of the results excluding one-time items show the same pattern; the magnitude of the temporal effects is attenuated but they remain sizable and statistically significant.

A separate investigation checks for the previously documented effect of rising incidence of losses over time (e.g.,Hayn 1995). Similar to special items, on one hand, the increasing incidence of losses could account for some of the documented patterns; on the other hand, the rising incidence of losses is itself a manifestation of worse matching, and thus, the interpretation of such tests and results has to be done with caution. Preliminary results confirm that our sample is generally less affected by loss-related effects because it comprises the largest firms, while loss incidence is heavily concentrated in small firms. In terms of actual results, the elimination of losses leads to a material attenuation of the documented pattern, although the attenuation is somewhat smaller than that for one-time items. Again, all main results remain statistically and economically significant.

Second, we investigate the effect of changing industry composition on our results, where industry composition can be viewed as a broad proxy for the changing nature of the economy and the firms over time. For example, if firms in certain industries tend to have more volatile and less persistent earnings, and these industries are becoming more prominent over time, then this could account for our results even if there is no *within-industry* change in the properties of earnings. This concern seems warranted given the large changes in industry composition in our sample over time, as illustrated in Table 6. In Table 6, we use the Fama-French 12-industry-group classification (obtained from http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html), and list industry firm count as of the first and last sample year, 1967 and 2003, respectively. An examination of Table 6 reveals dramatic temporal changes in industry composition, e.g., Consumer Non-Durables decreases from 13.75 percent to 5.46 percent of the sample, Manufacturing decreases from 26.99 percent to 11.54 percent of the sample, while Finance increases from 2.31 percent to 19.52 percent of the sample.

We investigate the effects of changing industry composition by re-running the main tests within two subsamples, one comprising industries that have increasing firm count over time and the other comprising industries that have decreasing firm count. The increasing subsample accounts for 29 percent of the firms at the beginning of the sample and 59 percent at the end, with the decreasing subsample accounting for 71 percent and 41 percent, respectively. If changing industry composition fully accounts for the documented results, then we expect to see substantial differences in average results across the two subsamples but little temporal variation in within-sample results. The actual results show the opposite pattern. We find that in all tests the results across the two subsamples show the same temporal patterns as the main tests above, i.e.,

TABLE 6

Changes in the Sample Composition over Time

Fama-French 12-Industry-Group Classification	**Beginning Sample Year = 1967** Count	Percentage	**Ending Sample Year = 2003** Count	Percentage
Consumer Non-Durables	107	13.75	52	5.46
Consumer Durables	40	5.14	27	2.83
Manufacturing	210	26.99	110	11.54
Energy	35	4.5	31	3.25
Chemicals and Allied Products	49	6.3	31	3.25
Business Equipment	44	5.66	85	8.92
Telecommunication	10	1.29	35	3.67
Utilities	119	15.3	142	14.9
Wholesale, Retail, and Some Services	65	8.35	87	9.13
Healthcare	23	2.96	51	5.35
Finance	18	2.31	186	19.52
Others—Mines, Construction, Transportation, and Business Service	58	7.46	116	12.17

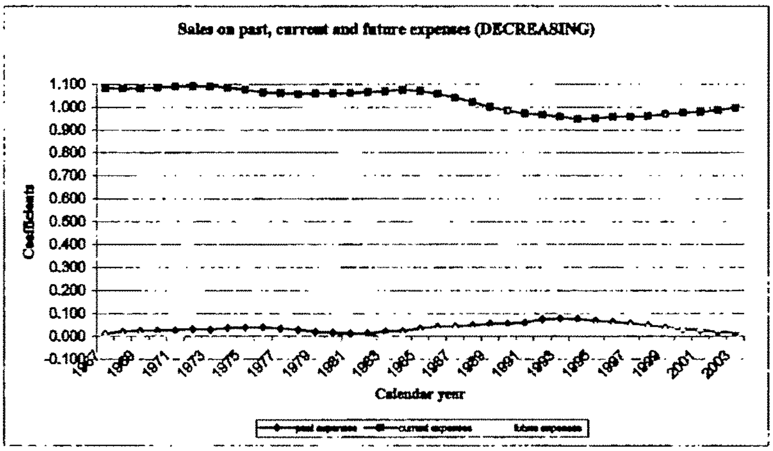
Details on the Fama-French 12-industry-group classification can be obtained from Kenneth French's personal webpage at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

decreasing correlation between sales and expenses, increasing volatility, and decreasing persistence of earnings. For parsimony, we omit most of the actual results and only offer Figure 4 as an illustration of the tenor of the results. Figure 4 corresponds to Figure 1 in the main results, only here the coefficients in regression of revenues on past, current, and future expenses are plotted for the two subsamples ("DECREASING" and "INCREASING"). The figure clearly demonstrates the similarity of results across the two subsamples, and statistical tests confirm at high levels of confidence that for both samples there are substantial declines in the correlation between revenues and contemporaneous expenses and substantial increases in the correlations between revenues and past and future expenses (actual results not tabulated). Additional tests reveal that the results are largely the same for industries, whose membership in the sample remains relatively constant (e.g., utilities, energy, and wholesale, retail, and some services). This evidence implies that industry composition and, more broadly, the evolution of the real economy are unlikely to fully account for the documented patterns in earnings.

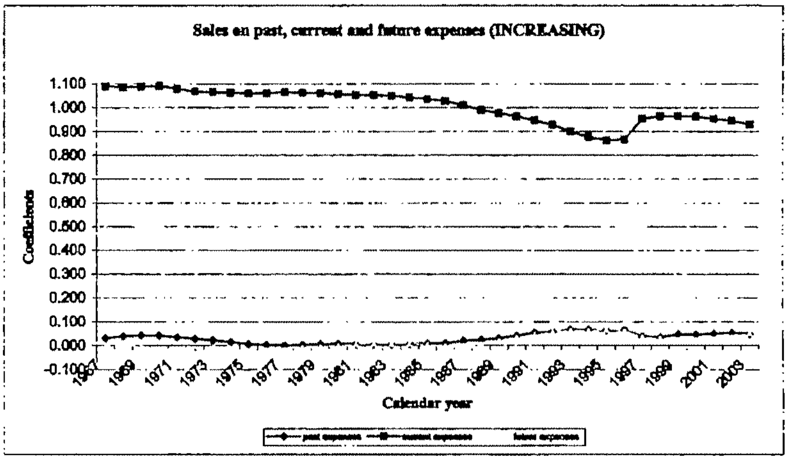
Third, we provide further evidence on the relative role of accounting and real economy factors on the observed temporal patterns in reported revenues, expenses and earnings by examining the temporal properties of *cash-based* measures of revenues, expenses, and earnings. If the real economy is the primary determinant of our main results, then one would expect to see similar patterns in cash-based measures, which are unaffected by the accrual process. Our measure of cash-based revenues is revenue minus changes in accounts receivable plus changes in deferred revenue, our measure of cash-based earnings is cash flow from operations (CFO), and cash expenses is defined as the difference between cash-based revenues and CFO. We use the balance-sheet method of deriving cash flow from operations, as in Sloan (1996), because our sample

FIGURE 4
Coefficients in Regression of Revenues on Past, Current, and Future Expenses by Industry Composition

Panel A: Coefficients in Regression of Revenue on Past, Current, and Future Expenses for the "DECREASING" Subsample



Panel B: Coefficients in Regression of Revenue on Past, Current, and Future Expenses for the "INCREASING" Subsample



Panel A plots the five-year moving average (the average for the current and previous four years) of slope coefficients of regression of current revenues on past, current, and future expenses for firms in industries whose proportion in the sample is decreasing over time. "DECREASING" industries include the following six industry groups: consumer non-durables, consumer durables, manufacturing, energy, chemical and allied products, and utilities.

Panel B plots the five-year moving average (the average for the current and the previous four years) of slope coefficients of regression of current revenues on past, current, and future expenses for firms in industries whose proportion in the sample is increasing over time. "INCREASING" industries include the following six industry groups: business equipment, telecommunication, wholesale and retail, health care, finance, and others, including mine, construction, transportation, and business service.

The division of the two subsamples is based on the change in industry composition as shown in Table 6. The overall sample starts from 1967 and ends on 2003. In order to limit the sample to economically substantial firms, we pick up the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available.

$Revenues_t$ is net revenues (Compustat item 12) deflated by average assets for the current period. $Expenses_t$ is the difference between *Revenues* and *Earnings* for the current period. $Expenses_{t-1}$ is the difference between *Revenues* and *Earnings* for the previous period. $Expenses_{t+1}$ is the difference between *Revenues* and *Earnings* for the next period. For each sample year, the regression of current revenues on past, current, and future expenses is run on a cross-sectional basis.

Details on the Fama-French 12-industry-group classification can be obtained from Kenneth French's personal webpage at: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

stretches considerably before 1988, when precise measures of cash flows from operations become available.

An examination of the cash-based results reveals none of the temporal trends identified in accrual-based results. For parsimony, we present only a subset of the complete results in Figure 5. Panel A of Figure 5 presents the coefficients from a regression of current cash revenues on past, present, and future cash expenses, and represents the cash counterpart of the accrual-based results in Figure 1. In contrast to the patterns in Figure 1, the coefficient on current cash expenses substantially rises over time and the coefficients on past and future cash expenses fall over time, and the mean difference on current cash expenses across the two subsample periods is statistically significant at the 0.001 level. This result indicates that, if anything, the documented patterns in Figure 1 likely understate the temporal deterioration in accounting matching. Panel B of Figure 5 presents the persistence in CFO and the autocorrelation CFO changes over time, the cash counterpart of Figure 3. Consistent with Dechow (1994), a comparison between Figure 3 and Panel B of Figure 5 reveals that CFO has lower persistence and higher autocorrelation in changes than accrual-based earnings.

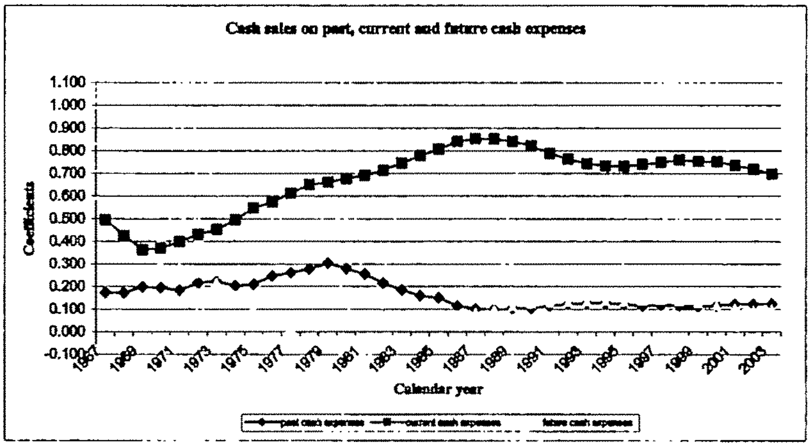
This comparison, however, also reveals dramatic differences in the temporal pattern of these two measures of performance. In contrast to Figure 3, the persistence of CFO and the autocorrelation of CFO changes remain largely the same over time. Specifically, the mean persistence of CFO is 0.440 and 0.472 for the two subsample periods, and the difference is not statistically significant (p-value = 0.359). Note also, that while in the beginning of the period the persistence of earnings is much larger than that of CFO (about 0.90 versus 0.45), by the end of the period this difference (about 0.65 versus 0.50) is eroded by two-thirds. Similar conclusions apply for the autocorrelation in CFO changes versus the autocorrelation in earnings changes. The combined impression from this evidence is that changes in the real economy play only a secondary role in explaining the changing properties of earnings over time.

In additional tests, we use the Richardson et al. (2005) accrual framework to split revenues and expenses into their cash and accrual components, and explore which of the interactions between these component variables is the chief driver of the documented decreased correlation between total revenues and expenses. We find that most of the pairwise correlations between the cash and accrual components decline over time, but find little reliable evidence of a dominant pair (actual results not included). Perhaps our most useful finding is that the correlation between the cash components of revenues and expenses is much higher than the correlation between the accrual components of revenues and expenses, while the variations in accrual revenues/total revenues and accrual expenses/total expenses have both increased substantially over time. Essentially, these results imply that revenues and expense determination today relies much more on accruals than in the past, while accrual components have lower correlation, driving the correlation between total revenues and expenses down. Further tests split the accrual component of expenses into working capital, long-term operating, and financing accruals and search for the differential roles of these components in explaining the observed relations. The results indicate that the variations in all of these accruals have increased over time but failed to uncover meaningful differences in their relative roles.

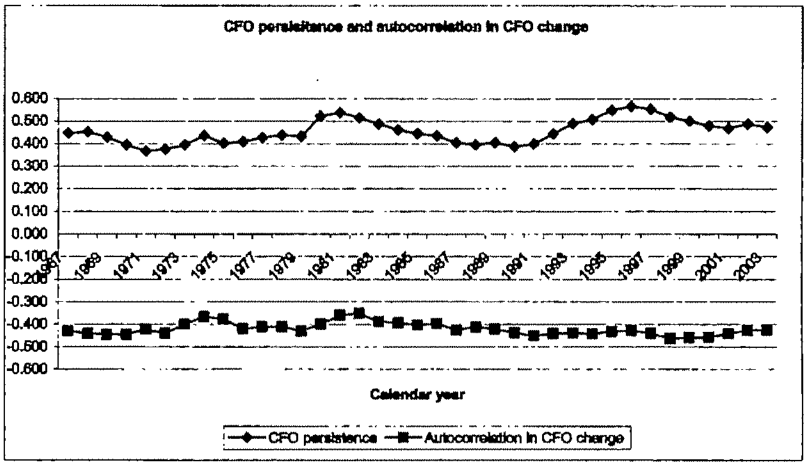
Finally, we re-run the main tests within terciles of accrual quality. Our motivation is that if accounting-related factors play a role in the temporal evolution of earnings characteristics, then we would find more pronounced results for firms where the quality of accruals and earnings is low because these firms are likely to be more affected by the general trend of deteriorating matching quality as compared to firms that have naturally good accrual quality. We use two proxies for accrual quality: absolute level of accruals based on Sloan (1996) and the residual accrual measure from Dechow and Dichev (2002). Since the results across these two measures are similar, we only discuss and present results for the Dechow and Dichev (2002) approach. We find that all results

FIGURE 5
Cash-Based Measures of Earnings, Revenues, and Expenses

Panel A: Coefficients in Regression of *Cash Revenues* on Past, Current, and Future Cash Expenses over Time



Panel B: *Cash Flows from Operations* (*CFOs*) Persistence and *Autocorrelation in CFO Changes* over Time



Panel A plots the five-year moving average (the average for the current and previous four years) of slope coefficients of regression of current cash revenues on past, current, and future cash expenses from year 1967 to year 2003.

Panel B plots the five-year moving average (the average for the current and the previous four years) of persistence in CFOs and autocorrelation in the change in CFOs.

The overall sample starts from 1967 and ends on 2003. In order to limit the sample to economically substantial firms, we pick up the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available.

Cash Flows from Operations is the difference between earnings (Compustat Item 18) and accruals as in Sloan (1996). More specifically, accruals is calculated as {the change in current assets (Compustat Item 4) – the change in cash (Compustat Item 1) – [change in current liabilities (Compustat Item 5) – the change in current debt (Compustat Item 34) – change in tax payable (Compustat Item 71)] – Depreciation (Compustat Item 14)}. *CFOs* is cash flows form operations deflated by average assets. *Cash Revenues* is net revenues (Compustat Item 12) minus changes in trade accounts receivables (Compustat Item 151) plus the change in deferred revenues (Compustat Item 356 + 397) deflated by average assets. *Cash Revenues*$_t$ is cash revenues deflated by average assets for the current period. *Cash Expenses*$_t$ is the difference between *Cash Revenues* and *CFOs* for the current period. *Cash Expenses*$_{t-1}$ is the difference between *Cash Revenues* and *CFOs* for the previous period. *Cash Expenses*$_{t+1}$ is the difference between *Cash Revenues* and *CFOs* for the next period. For each sample year, *Persistence in CFOs* is the slope coefficients from the regression of current CFOs on the previous period CFOs on a cross-section basis. *Autocorrelation in CFOs Change* is the cross-sectional correlation between current CFOs change and past CFOs change.

are more pronounced for low accrual quality terciles, consistent with our prediction. To avoid clutter, we only present the results for earnings volatility and its components in Figure 6, which contrasts the findings for the top and bottom terciles of accrual quality. An examination of Figure 6 reveals a clear difference in the temporal pattern of the results for these two subsamples. For firms with high accrual quality, the increase in earnings volatility starts late and is more moderate, with mean earnings volatility of 0.010 and 0.011 for the two subsample periods, and this difference is not statistically significant (p-value = 0.284). In contrast, for firms with low accrual quality, the increase in earnings volatility starts earlier and is more dramatic, with earnings volatility more than tripling by the end of the period. Mean earnings volatility increases substantially from 0.027 to 0.050 across the two subsample periods, and the difference is statistically significant (p-value = 0.001). Thus, these results confirm that accounting-related factors play a substantial role in the temporal trends of earnings characteristics.

Summarizing, the supplemental results are useful on two dimensions. First, they provide assurance that the main results are robust in a number of alternative specifications and relevant sample partitions. Second, the results suggest that accounting-based factors are a substantial and perhaps even primary determinant of the documented temporal patterns of earnings characteristics, while real-economy changes play a secondary role.

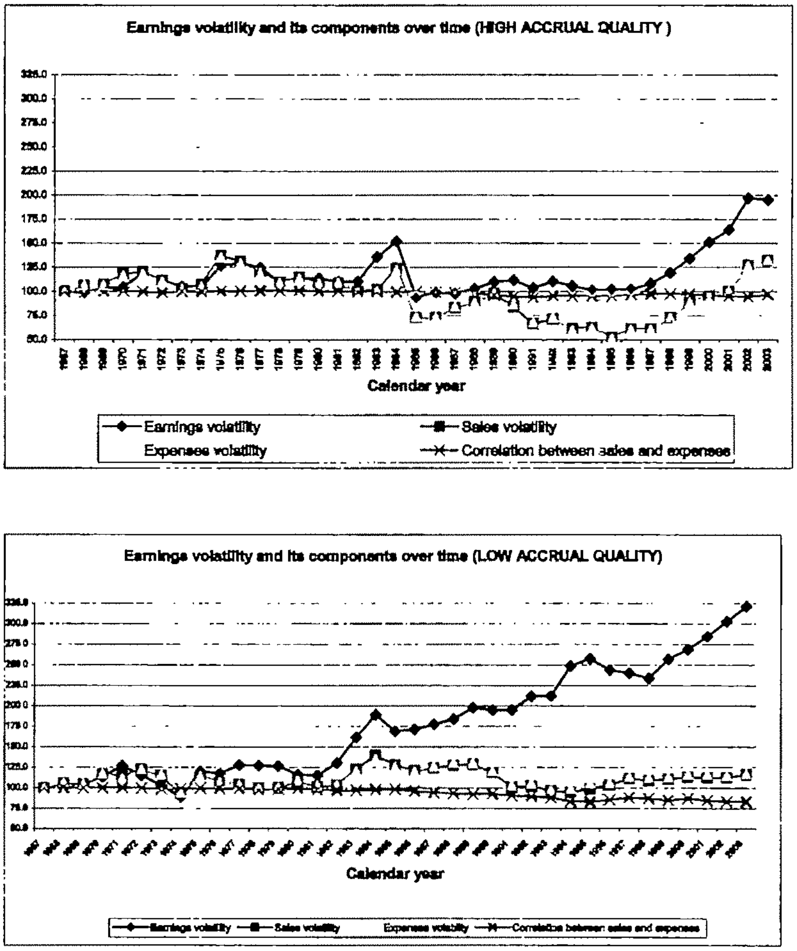
V. DISCUSSION OF THE RESULTS AND SUGGESTIONS FOR FUTURE RESEARCH

On the most immediate and obvious level, the results above serve to illustrate the economic importance of matching considerations for accounting earnings. The results indicate that poor matching leaves a large and perhaps decisive imprint on all key properties of earnings, including persistence, volatility, and reversibility of changes. Since earnings is the most widely used accounting variable, these results suggest that a consideration of matching quality can bring useful insights to users. Note that the empirical analysis in this study is limited to a large-firm time-series specification, while much more can be done exploring cross-sectional specifications. For example, Dichev and Tang (2008) use a matching motivation to link earnings volatility to practical earnings predictability.

The results in this study also seem useful as a plausible explanation for the pronounced temporal decline in the informativeness of earnings, e.g., Collins et al. (1997) find that the sensitivity of stock prices to earnings has substantially declined over time and Francis and Schipper (1999) find that the stock return benefits from perfect foreknowledge of earnings have diminished over time. Various explanations for this phenomenon have been advanced, for example, the increased role of technology and the proliferation of transactions and activities, which are difficult to capture in the traditional financial reporting model. However, the success of these explanations has been limited (Francis and Schipper 1999). The results in this study suggest an intuitive alternative story for the decline in the informativeness of earnings. The evidence suggests that matching has become worse over time and earnings have become more volatile and less persistent. Since there is strong evidence that stock prices are less sensitive to less persistent earnings (e.g., Collins and Kothari 1989), there seems to be a natural connection between the temporal declines in the quality of matching and the informativeness of the resulting earnings. Of course, at this point this connection is just a conjecture, which needs to be more carefully evaluated in future research.

Apart from the immediate and more clearly delineated research-related implications, the results of this study provide import for larger and more philosophical themes. For example, the results in this study can be useful in FASB's and IASB's considerations. They show that standard-setters' deliberate and far-reaching move away from matching as the foundation of financial reporting and toward a balance-sheet-based model has likely had a pronounced effect on the properties of earnings. Of course, whether this effect is considered desirable or undesirable

FIGURE 6
Earnings Volatility and Its Components Conditional on HIGH and LOW ACCRUAL QUALITY



This figure plots the normalized earnings volatility, revenues volatility, expenses volatility and the correlation between revenues and expenses conditional on the quality of accruals.

The sample starts from 1967 and ends on 2003. In order to limit the sample to economically substantial firms, we pick up the largest 1,000 firms for each year in the sample where volatility in two-year earnings, volatility in two-year revenues, and volatility in two-year expenses are available. The quality of accruals is the residual accrual measure from Dechow and Dichev (2002). More specifically, for each firm-year observation, we use the prior ten firm-year observations to estimate the following regression:

$$\Delta WC_{i,t} = \alpha + \beta_1^* CFOs_{i,t-1} + \beta_2^* CFOs_{i,t} + \beta_3^* CFOs_{i,t+1} + \varepsilon_{i,t}$$

The resulting standard deviation of $\varepsilon_{i,t}$ is the measure of accrual quality for the specific firm-year observation. For each sample year, we partition the whole sample into three groups based on accrual quality. The figure plots earnings volatility and its components over time for firms within the highest accrual quality tercile and for firms within the lowest accrual quality tercile respectively.

ΔWC is calculated as {the change in current assets (Compustat Item 4) – the change in cash (Compustat Item 1) – [the change in current liabilities (Compustat Item 5) – the change in current debt (Compustat Item 34) – the change in tax payable (Compustat Item 71)]}. *Cash Flows from Operations* is the difference between earnings (Compustat Item 18) and accruals as in Sloan (1996). More specifically, accruals is calculated as {the change in current assets (Compustat Item 4) – the change in cash (Compustat Item 1) – [change in current liabilities (Compustat Item 5) – the change in current debt (Compustat Item 34) – change in tax payable (Compustat Item 71)] – Depreciation (Compustat Item 14)}. $CFOs_t$ is the cash flows from operations deflated by average assets for the current period. $CFOs_{t-1}$ is the cash flows from operations deflated by average assets for the previous period. $CFOs_{t+1}$ is the cash flows from operations deflated by average assets for the future period. *Earnings* is earnings before extraordinary items (Compustat Item 18) deflated by average assets (Item 6). *Revenues* is net revenues (Compustat Item 12) deflated by average assets. *Expenses* is the difference between *Revenues* and *Earnings*. *Vol* (*Earnings*) is earnings volatility, which is calculated by taking the standard deviation of the deflated earnings for the most recent five years. *Vol* (*Revenues*) is revenues volatility, which is calculated by taking the standard deviation of the deflated revenues for the most recent five years. *Vol* (*Expenses*) is expenses volatility, which is calculated by taking the standard deviation of the deflated expenses for the most recent five years. *Corr* (*Revenues, Expenses*) is the correlation between revenues and expenses, which is calculated as the correlation between the deflated revenues and the deflated expenses for the most recent five years. Normalize all volatility and correlation variables by their values for the beginning year of 1967.

depends on the goals of the financial reporting system, and indeed there are good reasons for why a balance sheet model may be preferred, e.g., FASB's avowed emphasis on solid conceptual foundations and the demands of auditors and lawyers for verifiable and enforceable claims to asset-based wealth. However, it is important to consider that these gains have been achieved at the expense of the informativeness of earnings, which is still widely considered the most important output of the accounting system, especially for the needs of equity investors (Graham et al. 2005).

The problem in a nutshell is this. If investors should "buy earnings," as many of the classics in investment advise, then the key value metric is the long-run earnings power of the company. If accounting is dedicated to proper matching, then the resulting earnings would be a fairly good representation of the long-run economic success of advancing expenses to produce revenues. Thus, properly matched earnings will have only economics-driven and comparatively little volatility, substantial persistence, and little reversibility in changes. In such a world, current earnings will be the best starting point for the prediction of earnings, and indeed it will be difficult to improve on their predictive ability by using other information. Such a world is far from a fantasy; in fact, the results in this study and other research like Ball and Watts (1972) indicate that real-world earnings had exactly these characteristics in the late 1960s and early 1970s. However, since then the situation has radically changed. Using the results of this study, earnings persistence for the largest 1,000 U.S. firms has declined from 0.91 in 1967 to 0.65 in 2003—a massive, seismic change. If this trend continues unabated for another 30 to 50 years, then the persistence and forward-looking informativeness of earnings will be simply destroyed. This seems like a very real possibility considering that the FASB (and to a lesser extent the IASB) has been steadily increasing the scope of its balance-sheet-based accounting, most notably with its recent efforts to expand mark-to-market accounting. It is useful to consider that pure mark-to-market accounting produces asset and liability revaluations (or earnings) that are very volatile and have zero persistence, and that such earnings will be of little forward-looking use to investors. At a minimum, it seems that standard-setters should give a more explicit consideration to these very real costs of creeping earnings irrelevance in their deliberations about the ultimate scope of balance-sheet-based accounting.

Of course, as discussed earlier, the powerful temporal trend of declining informativeness of earnings is probably not only a result of the evolution of standard-setting, but also of changes in the real economy as well, and there is little that financial reporting can do about the nature of these changes per se. However, the evidence presented in this study suggests that changes in the real economy have played a secondary role in the evolution of the properties of earnings. In addition, if the question is "What can be done to counter the effect of these changes on the informativeness of earnings," then the answer and the discretion again lie in the design of the financial reporting system.

Whether and when standard-setters will take such arguments into consideration is not clear since balance-sheet-based accounting has its own considerable momentum now. However, what is safer to say is that the secular decline in the forward-looking informativeness of earnings is a material fact, which will unavoidably bring forth reactions from various users of financial information. Some such reactions are already underway. For example, the proliferation of "pro forma" earnings in the 1990s can be viewed as a somewhat instinctive and disorderly attempt to provide the important distinction between persistent and recurring components of earnings from sporadic and non-recurring components. It is instructive to consider that even after the recent crackdown on pro forma earnings and the ensuing greater cost in using them, most preparers and users of financial information have modified but not abandoned their use (Marques 2006). This experience suggests that the use of modified definitions of earnings serves a solid reason and it is here to stay and evolve in spite of chaotic beginnings. This is one place where academic research can make a difference and lead the way, e.g., see Ohlson (2006) for a proposal for a conceptually sound and practical definition of pro forma earnings. The evidence in this study also provides some clues about possible alternative definitions of earnings. The evidence on longer-horizon earnings suggests that the consideration of the historical record of earnings is becoming more important than in earlier years when earnings behavior was close to random walk. The evidence that there is a strong reversibility in earnings changes suggests that various smoothing techniques could be useful in the specification of more informative earnings, a prescription similar in spirit to Ohlson (2006). These and other possibilities await future research.

VI. SUMMARY AND CONCLUSION

This study presents a theory of matching and its effects on accounting variables and especially earnings. The principal insight of the theory is that poor matching acts as "noise" in the economic relation of advancing expenses to earn revenues, and thus poor matching decreases the contemporaneous correlation between revenues and expenses. As a result, poor matching increases the volatility of earnings, decreases the persistence of earnings, and induces more negative autocorrelation in earnings changes.

Our empirical results are strongly consistent with the conjecture that matching is becoming worse as a result of the evolution in accounting and the real economy over time. We find a clear decrease in the correlation between contemporaneous revenues and expenses and an increase in the non-contemporaneous correlation between revenues and expenses. The volatility of reported earnings has doubled, while the underlying volatilities of revenues and expenses have remained largely the same. There is a stark decline in the persistence of earnings, where persistence starts at 0.91 at the beginning of our sample and is down to 0.65 by the end. The first-order autocorrelation in earnings changes hovers near zero at the beginning of the sample, and is about −0.30 at the end. In contrast, there are none of these temporal patterns in cash-based measures of revenues, expenses, and earnings.

As discussed at more length in the paper, these results have a number of implications and can serve as a springboard for future research. One implication is that matching considerations have a pronounced effect on the properties of earnings, and one can use quality of matching measures to pursue quality of earnings investigations, possibly including earnings forecasting and equity valuation. Another implication is that standard-setters' embrace of a balance-sheet-based model of financial reporting has reduced the forward-looking informativeness of earnings, and this presents a real difficulty and cost to many users of financial reports. Last, these results imply that the need for more informative earnings is real, and will be growing, so research into various improved re-formulations of earnings is warranted.

APPENDIX

Proof of Observation 1

The proof for the contemporaneous relation is given by considering the definition of correlation under perfect and poor matching and having in mind that mismatched expenses are uncorrelated with contemporary well-matched revenues and expenses:

Perfect matching: $\mathrm{Corr}(Rev_t^*, Exp_t^*) = \mathrm{Cov}(Rev_t^*, Exp_t^*) / \{\mathrm{Std}(Rev_t^*) * \mathrm{Std}(Exp_t^*)\}$
Poor matching: $\mathrm{Corr}(Rev_t^*, Exp_t) = \mathrm{Cov}(Rev_t^*, Exp_t^* + \nu_t) /$
$\{\mathrm{Std}(Rev_t^*) * \mathrm{Std}(Exp_t^* + \nu_t)\} = \mathrm{Cov}(Rev_t^*, Exp_t^*) / \{\mathrm{Std}(Rev_t^*) * \mathrm{Std}(Exp_t^* + \nu_t)\}$

Since the numerators are identical and the denominator for bad matching is larger and increasing in the variance of ν, it follows that poor matching decreases the contemporaneous correlation between revenues and expenses.

Proof of Corollary to Observation 3

The proof for a negative autocorrelation in earnings changes is given by expressing autocorrelation in earnings changes in terms of persistence coefficient.

Perfect matching:
$E^*_{t+1} = \beta^*_0 + \beta^*_1 E^*_t + \varepsilon_t$
By definition, $\Delta E^*_{t+1} = E^*_{t+1} - E^*_t = \beta^*_0 + \beta^*_1 E^*_t + \varepsilon_{t+1} - E^*_t$
$= \beta^*_0 + (\beta^*_1 - 1)E^*_t + \varepsilon_{t+1}$
Similarly, $\Delta E^*_t = E^*_t - E^*_{t-1} = \beta^*_0 + (\beta^*_1 - 1)E^*_{t-1} + \varepsilon_t$
$\text{Autocorrelation}(\Delta E^*_{t+1}, \Delta E^*_t) = \text{Cov}(\Delta E^*_{t+1}, \Delta E^*_t) / [\text{Std}(\Delta E^*_{t+1}) {}^* \text{Std}(\Delta E^*_t)]$
$= \text{Cov}\{\beta^*_0 + (\beta^*_1 - 1)E^*_t + \varepsilon_{t+1}, \beta^*_0 + (\beta^*_1 - 1)E^*_{t-1} + \varepsilon_t\} / \{\text{Std}(\Delta E^*_{t+1}) {}^* \text{Std}(\Delta E^*_t)\}$
Plug in Equation (5) in the body of text: $\text{Var}(E^*_t) = \text{Var}(\varepsilon)^*(1 + \beta^{*2}_1 + \beta^{*4}_1 + \dots)$ and $\lim\{\text{Var}(E^*_t)\} = \text{Var}(\varepsilon) / (1 - \beta^{*2}_1)$ into the autocorrelation formula yields:
$\text{Autocorrelation } (\Delta E^*_{t+1}, \Delta E^*_t) = \{\beta^*_1(\beta^*_1 - 1)^2 * \text{Var}(\varepsilon) / (1 - \beta^{*2}_1) + (\beta^*_1 - 1)\text{Var}(\varepsilon)\} /$
$\{[(\beta^*_1 - 1)^2 / (1 - \beta^{*2}_1) + 1] * \text{Var}(\varepsilon)\} = (\beta^*_1 - 1) / 2$
Given that under perfect matching $E^*_t = \beta^*_0 + \beta^*_1 E^*_{t-1} + \varepsilon_t$
$\text{Autocorrelation } (\Delta E^*_{t+1}, \Delta E^*_t) = (\beta^*_1 - 1) / 2$
Poor matching:
Given that under poor matching $E_t = \beta_0 + \beta_1 E_{t-1} + \varepsilon_t$
$\text{Autocorrelation } (\Delta E_{t+1}, \Delta E_t) = (\beta_1 - 1) / 2$

From observation 3, persistence coefficient for earnings under poor matching (β_1) is lower than the persistence coefficient for earnings under perfect matching (β^*_1). Based on the relationship between earnings persistence in earnings and the autocorrelation in earnings changes, it follows that the autocorrelation is more negative under poor matching than under perfect matching.

Proof of Observation 4

The proof that the effects of poor matching are attenuated over longer horizons is given by considering that accounting is self-correcting and therefore by their nature mismatched errors resolve over longer horizons. More specifically, assume mismatched error is corrected in the next period, in an n-year horizon, all mismatched errors in the middle n–2 of the n years are resolved and only some of the mismatched errors in the first and the last years remain.

Let *E*(*N*), *REV*(*N*), and *EXP*(*N*) denote earnings, revenues, and expenses over longer horizons, where $N > \, = 2$. By definition:

Under perfect matching:
$Rev(N)^*_t = 1 / N^*\Sigma(Rev^*_t + Rev^*_{t+1} \dots + Rev^*_{t+N-1})$
$Exp(N)^*_t = 1 / N^*\Sigma(Exp^*_t + Exp^*_{t+1} \dots + Exp^*_{t+N-1})$
$E(N)^*_t = 1 / N^*\Sigma(E^*_t + E^*_{t+1} \dots + E^*_{t+N-1})$

Under poor matching:
$Rev(N)_t = 1 / N^*\Sigma(Rev^*_t + Rev^*_{t+1} \dots + Rev^*_{t+N-1}) = Rev(N)^*_t$
$Exp(N)_t = 1 / N^*\Sigma(Exp_t + Exp_{t+1} \dots + Exp_{t+N-1})$
$= 1 / N^*\Sigma(Exp^*_t + \tau_t - \tau_{t-1} + Exp^*_{t+1} + \tau_{t+1} - \tau_t \dots + Exp^*_{t+N-1} + \tau_{t+N-1} - \tau_{t+N-2})$
$= 1 / N^*\Sigma(Exp^*_t + Exp^*_{t+1} \dots + Exp^*_{t+N-1} + \tau_{t+N-1} - \tau_{t-1})$
$= 1 / N^*\Sigma(Exp^*_t + Exp^*_{t+1} \dots + Exp^*_{t+N-1}) + 1 / N^*(\tau_{t+N-1} - \tau_{t-1})$
$= Exp(N)^*_t + 1 / N^*(\tau_{t+N-1} - \tau_{t-1})$
$E(N)_t = 1 / N^*\Sigma(E_t + E_{t+1} \dots + E_{t+N-1})$
$= 1 / N^*\Sigma(E^*_t - \tau_t + \tau_{t-1} + E^*_{t+1} - \tau_{t+1} + \tau_t \dots + E^*_{t+N-1} - \tau_{t+N-1} + \tau_{t+N-2})$
$= 1 / N^*\Sigma(E^*_t + E^*_{t+1} \dots + E^*_{t+N-1} - \tau_{t+N-1} + \tau_{t-1})$

$=1 / N^{*}\Sigma(E_t^* + E_{t+1}^* \ldots + E_{t+N-1}^*) + 1 / N^{*}(-\tau_{t+N-1} + \tau_{t-1})$
$=E(N)_t^* + 1 / N^{*}(-\tau_{t+N-1} + \tau_{t-1})$

Observation 1: Contemporaneous Correlation between Revenues and Expenses

As demonstrated in proof of observation 1, the effect of poor matching on contemporary matching is caused by the larger variance in mismatched expenses and is increasing in the difference between the variance of mismatched expenses and the variance of perfectly matched expenses.

For one-year horizon:
$\mathrm{Var}(Exp_t) = \mathrm{Var}(Exp_t^* + \tau_t - \tau_{t-1}) = \mathrm{Var}(Exp_t^*) + \mathrm{Var}(\tau_t - \tau_{t-1})$
$=\mathrm{Var}(Exp_t^*) + 2\mathrm{Var}(\tau)$
For N-year horizon:
$\mathrm{Var}(Exp(N)_t) = \mathrm{Var}\{Exp(N)_t^* + 1 / N^{*}(-\tau_{t-1} + \tau_{t+N-1})\}$
$=\mathrm{Var}(Exp(N)_t^*) + 1 / N^2 * 2\mathrm{Var}(\tau)$

Compared with "perfectly matched" one-year expenses, the volatility of reported one-year expenses increase by $2\mathrm{Var}(\tau)$. Compared with "perfectly matched" N-year earnings, the volatility of reported N-year expenses increase by $1 / N^2 * 2\mathrm{Var}(\tau)$. As N becomes larger, the increase becomes smaller (as N approaches infinity, the difference in variance in expenses under perfect matching and poor matching approaches zero). It follows that as the horizon expands, the effect of poor matching on contemporaneous correlation between revenue and expenses is attenuated.

Observation 2: Volatility of Reported Earnings

For one-year horizon:
$\mathrm{Var}(E_t) = \mathrm{Var}(E_t^* + \tau_{t-1} - \tau_t) = \mathrm{Var}(E_t^*) + \mathrm{Var}(\tau_{t-1} - \tau_t) + 2\mathrm{cov}(E_t^*, \tau_{t-1} - \tau_t)$
$=\mathrm{Var}(E_t^*) + \mathrm{Var}(\tau_{t-1} - \tau_t) = \mathrm{Var}(E_t^*) + 2\mathrm{Var}(\tau)$
For N-year horizon:
$\mathrm{Var}(E(N)_t) = \mathrm{Var}(E(N)_t^* + 1 / N^{*}(\tau_{t-1} - \tau_{t+N-1}))$
$=\mathrm{Var}(E(N)_t^*) + 1 / N^{*}\mathrm{Var}(\tau_{t-1} - \tau_{t+N-1})$
$=\mathrm{Var}(E(N)_t^*) + 1 / N^{2*}2\mathrm{Var}(\tau)$

Compared with "perfectly matched" earnings, the volatility of reported one-year earnings increase by $2\mathrm{Var}(\tau)$. Compared with "perfectly matched" earnings, the volatility of reported N-year earnings increase by $1 / N^{2*}2\mathrm{Var}(\tau)$. The increase in the volatility of N-year mismatched earnings is only $1 / N^2$ of that for one-year mismatched earnings. As N approaches zero, the difference between perfectly matched N-year earnings and mismatched N-year earnings approaches zero. It follows that the increase in the volatility of reported earnings induced by poor matching is attenuated in longer horizons.

Observation 3: Persistence Coefficient and Autocorrelation in Earnings Changes

One-year horizon:
From Equation (10) in the body of text, OLS estimation produces a biased and inconsistent estimate of β_1^*:

$$\beta_1^{OLS} = \beta_1^* + \frac{\sum E'_{t-1}\lambda'}{\sum E'^{2}_{t-1}}$$

where $\lambda = \varepsilon_t - \tau_t + (1 + \beta_1^*)\tau_{t-1} - \beta_1^*\tau_{t-2}$.
As the error term (λ) is correlated with the independent variable, the magnitude in the bias

induced by poor matching is

$$\frac{\sum E'_{t-1}\lambda'}{\sum E'^{2}_{t-1}}.$$

N-year horizon:

$$E(N)_t - v(N)_t = \beta(N)^*_0 + \beta(N)^*_1(E(N)_{t-1} - v(N)_{t-1}) + \varepsilon(N)_t,$$

$$E(N)_t = \beta(N)^*_0 + \beta(N)^*_1 E(N)_{t-1} + \{\ \varepsilon(N)_t - \beta(N)^*_1 v(N)_{t-1} + v(N)_t)\}$$

Rearrange:

$$E(N)_t = \beta(N)^*_0 + \beta(N)^*_1 E(N)_{t-1} + \{\varepsilon(N)_t - \beta(N)^*_1 1/N(-\tau_{t+N-2} + \tau_{t-2}) \\ +1/N(-\tau_{t+N-1} + \tau_{t-1})\}$$

Note that the second term in the error term is negatively correlated with $E(N)_{t-1}$ (since $E(N)_{t-1} = \mathrm{E}(N)^*_{t-1} + 1/N(-\tau_{t+N-2} + \tau_{t-2})$, the OLS estimator is given by the following expression:

$$\mathrm{B}(N)^{\mathrm{OLS}}_1 = \beta(N)^*_1 + \frac{\sum E(N)'_{t-1}\lambda(N)'}{\sum E(N)'^{2}_{t-1}}$$

where $\lambda(N) = \varepsilon(N)_t - \beta(N)_1/N(\tau_{t-2} - \tau_{t+N-2}) + 1/N(-\tau_{t+N-1} + \tau_{t-1})$

$$\frac{\sum E(N)'_{t-1}\lambda(N)'}{\sum E(N)'^{2}_{t-1}} = \frac{\sum E'_{t-1}\lambda'}{N\ \mathrm{var}(E_{t-1})} < \frac{\sum E'_{t-1}\lambda'}{\sum E'^{2}_{t-1}}$$

The bias in N-year horizon is smaller than the bias in one-year horizon. As N approaches infinity, the bias approaches zero. It follows that the attenuation in persistence coefficient of earnings induced by poor matching is alleviated in longer horizons. As the autocorrelation in earnings changes is the other side of earnings persistence, it follows that the autocorrelation in earnings changes gets less negative over longer horizon.

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Reducing Management's Influence on Auditors' Judgments: An Experimental Investigation of SOX 404 Assessments

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ABSTRACT: Auditors often receive summary information or conclusions from management about account balances or internal controls. They must then gather evidence to assess whether this information is fairly stated. In such situations, management can be considered the "first mover" and the auditor the "second mover." When auditors are the second mover, they are vulnerable to the curse of knowledge bias—the inability to ignore previously processed information (Fischhoff 1977). Specifically, because information from management could be incorrect or biased, auditors must arrive at an independent evaluation of the item in question (e.g., year-end book values, accounting estimates, or internal controls). This study examines the general issue of auditors being "second movers" by investigating how their awareness of management's severity classifications of internal control problems influences auditors' initial assessments of internal control over financial reporting (ICFR) under Auditing Standard No. 2. Our experimental design allows us to determine that management's "first mover" influence on auditors' judgments is an unintentional cognitive effect, rather than an intentional use of management's classifications. We further examine whether cognitively restructuring the ICFR assessment task reduces management's influence on auditors' judgments by asking auditors to evaluate and explicitly document the likelihood and magnitude of the effect of an ICFR problem on the financial statements. We find that cognitively restructuring the task mitigates management's "first mover" influence on auditors' judgments.

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I. INTRODUCTION

Auditors are increasingly required to solve complex, unstructured audit problems for which there is not a well-defined solution path and there is limited feedback to facilitate learning. In these situations, a practical audit approach is to use client-prepared data as a starting point and test whether that data is fairly stated. In such cases, where management is the "first mover" and auditors are in the "second mover" position, there is a risk that auditors' judgments will be biased in the direction of management's judgments.

Some examples of audit tasks for which management generates summary information and auditors are the "second mover" and may not independently arrive at their own judgments are: auditing the fair value of client assets, conducting substantive analytical procedures, and auditing management's accounting estimates. In some cases (such as fair value estimates), auditing standards recommend that auditors test the process that management used to generate the unaudited balances (e.g., see International Standard on Auditing 540, IAASB 2008). However, in other instances, auditors test whether management's represented value, estimate, or categorization is reasonable, instead of making an independent determination, because they believe it is more efficient to do so.[1] When auditors receive management's assessment or summary information before making an independent evaluation of the evidence, they are vulnerable to the curse of knowledge bias, or the inability to ignore previously processed information (Fischhoff 1977).

In the current paper, we examine the general issue of auditor susceptibility to the curse of knowledge bias due to being the "second mover" by focusing on the auditors' initial evaluations of Internal Controls Over Financial Reporting (ICFR) as required under the Sarbanes-Oxley Act of 2002 (SOX, U.S. House of Representatives 2002). SOX Section 404 requires a company's management and its auditors to document, test, and evaluate the effectiveness of ICFR, and each to reach independent conclusions regarding the severity of any control problems found. Management is typically the first mover in this process because completing its ICFR testing and evaluation before the audit often allows the company to remedy any material weaknesses before the end of the fiscal year under audit to avoid the negative public disclosure of the adverse opinion. Management could also conduct its ICFR testing and evaluation earlier in the fiscal year to comply with the quarterly certification process required under SOX Section 302.[2] The auditor's knowledge of management's initial classification of an ICFR problem as a control deficiency, significant deficiency, or material weakness could have an impact on the auditor's subsequent testing and evaluations. Ultimately, being influenced by management's classification could result in the auditor's final assessment being biased in the direction of management's classifications.

The ICFR classification is an important and high-stakes judgment because finding even one material weakness requires the auditor to issue an adverse opinion on the client's ICFR. Indeed, over the past several years, SOX 404 testwork has resulted in numerous companies disclosing material weaknesses in their annual report filings (Ge and McVay 2005; Jonas et al. 2005). Further, auditors' evaluations of their clients control weaknesses can have important consequences for management. Recent evidence on the impact of disclosures of material internal control weaknesses finds that management, specifically the chief financial officer, is likely to be replaced within six months of such disclosures (Banham 2006) and that there is a higher incidence of auditor resignation associated with such disclosures (Ashbaugh-Skaife et al. 2007). Therefore, management has an incentive to downplay the severity of any control problems identified, and auditors should exhibit professional skepticism and, thus, should not be influenced by management's classification, especially when it is self-serving.

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[1] Auditors might use management data as a starting point because they are reacting to time pressure. However, there are also situations where there might be numerous approaches to arrive at an unaudited estimate or value and it would not be feasible to test them all.

[2] Based on our discussions with auditors, management performs its ICFR testing as early as the first quarter to facilitate the quarterly certification that it must make to regulators, in addition to identifying problems that can be remedied. Of course, the goal would be for management to remedy all problems before year-end so that none would need to be reported. However, this is often not possible because some problems cannot be remedied within this limited time frame (e.g., need to hire a new accounting staff).

Prior literature (e.g., McDaniel and Kinney 1995; Kennedy 1995) finds that auditors are influenced by having management's summary data or conclusions available to them before making their own judgments. In the settings examined in these prior studies, despite being swayed by management's information, the auditor could ultimately receive feedback about the actual outcome. However, in more unstructured tasks like fair value estimation or ICFR classifications, the actual outcome or true state might not be available at the time of the audit report. Indeed, if no misstatements are detected by the auditor or the client, then the severity of an ICFR problem might never be detected.

In the current study, we conduct two related experiments. The first experiment tests whether auditors exhibit a curse of knowledge bias in their initial judgments of the severity of an ICFR problem.[3] That is, can auditors ignore the ICFR classifications provided by management when they make their own, independent assessment? Our first experiment is also designed to distinguish whether the potential influence of management's classification on auditor judgment is due to an unintentional cognitive bias and/or the intentional use of management's classification.

Kennedy (1995) notes that it is extremely difficult to reduce the curse of knowledge bias because it is a hard-wired cognitive effect that is not mitigated by just trying harder. Kennedy (1995) suggests that the best way to remedy a hard-wired bias is to cognitively restructure the task. Therefore, our second experiment examines the potential remediating effects of cognitively restructuring the ICFR task. Specifically, we examine whether instructing auditors to explicitly consider and document the potential financial statement impact of ICFR problems helps reduce any curse of knowledge effect.

While both Auditing Standard No. 2 (AS No. 2, PCAOB 2004a) and its replacement Auditing Standard No. 5 (AS No. 5, PCAOB 2007) require auditors to identify such an impact on financial statements, neither these standards nor the current PCAOB documentation standard, Auditing Standard No. 3 (AS No. 3, PCAOB 2004b), specifically require that auditors document this process. Thus, there is uncertainty about whether auditors consistently engage in the reasoning process necessary to determine the possible effect of each problem on the financial statements. Therefore, our second experiment tests whether the instruction to evaluate and explicitly document the possible impact of the ICFR problem on the financial statements can help auditors overcome the curse of knowledge bias.

Results indicate that auditors' judgments were biased by prior knowledge of management's classification. Further, auditors' judgments were influenced by management's classification in the situation where auditors should have been most skeptical, i.e., when management's classification understated the severity of the ICFR problem. We also found that restructuring the task by asking auditors to evaluate and explicitly document the financial statement impact of the ICFR problem reduced the influence of management's classifications on their judgments.

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[3] In this study we examine the initial stage of the auditors' ICFR evaluation before any audit procedures and testwork are performed.

The remainder of this paper is organized as follows: in Section II we provide an overview of the requirements of PCAOB Auditing Standards, as well as a description of the theory and hypotheses tested. Section III outlines the experimental method and Section IV presents the results. Discussion and conclusions are provided in Section V.

II. THEORY AND HYPOTHESES

Background—Evaluating Internal Control Deficiencies

PCAOB standards outline auditors' responsibility when auditing management's assertions regarding its internal controls. Management must make an assessment of its own internal controls. Then auditors must also evaluate the effectiveness of the control system in preventing financial statement misstatement (AS No. 2, paragraph 5; AS No. 5, 16).[4] If there are no "material weaknesses" identified, and no scope restrictions on the audit, then the auditor could issue an unqualified opinion (AS No. 2, paragraph 129). However, if the internal control system contains even one "material weakness," then the auditor must issue an adverse opinion on the effectiveness of the internal control over financial reporting (AS No. 2, paragraph 175).

ICFR problems that do not rise to the level of a material weakness should be classified as either significant deficiencies or control deficiencies (under AS No. 2).[5] Although a significant deficiency should not be considered material in isolation, when it is aggregated with other deficiencies, or if it persists unremediated over time, it should be considered a strong indicator of a material weakness (BDO Seidman et al. 2004; AS No. 5, paragraph 79).

Auditing standards generally require auditors to reach judgments independent of management. The ICFR standards require an *independent* assessment of the client's internal control system when classifying ICFR problems and determining whether they reach the level of a significant deficiency or a material weakness (e.g., AS No. 2, paragraphs 108, 110, 111).[6] However, previous literature in psychology and accounting suggest that it is likely that auditors will be biased by receiving management's classification before arriving at their own independent judgments. While auditors might believe that starting with management's classifications helps them to reduce their audit hours (i.e., improves their efficiency), it is likely that it also biases their judgments, thereby reducing their audit effectiveness.

The First Mover Effect of Receiving Management's Judgment before Making an Independent Judgment

Prior research in both psychology and auditing suggests that auditors' judgments will be biased by receiving management's classification of the ICFR problem before making their own judgments because auditors are vulnerable to the curse of knowledge. Citing Fischhoff's seminal work in this area (e.g., Fischhoff 1977), Kennedy (1995, 249) describes the curse of knowledge bias as "being unable to disregard information already processed"[7] and notes that the curse of knowledge is not an effort-related bias. Rather, it is a hard-wired data-related bias that can only be removed with a cognitive intervention (Kennedy 1995, 253). Kennedy (1995) finds that the curse of knowledge bias exists in a going-concern judgment task and that effort-related interventions like accountability are not effective in mitigating curse of knowledge effects. Her study determined that neither experience nor the timing of when participants were informed that they would have to justify their responses reduced the bias.

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[4] The current study was conducted when AS No. 2 was in place. However, AS No. 5 still applies to all aspects of the process that we examine. Under AS No. 5, auditors must assess the client's internal controls and judge the severity of any deficiencies in a way that is very similar to that described in AS No. 2. Because in AS No. 2, auditors found the wording of the definitions of the three categories of ICFR problems to be confusing, in AS No. 5 the definitions were changed. However, despite the change in wording, the three categories essentially have the same meaning under both standards. According to AS No. 5, "a deficiency in control (CD) exists when the design or operation of a control does not allow management or employees in the normal course of performing their assigned functions, to prevent or detect misstatements on a timely basis." A significant deficiency (SD) is defined as "a deficiency or combination of deficiencies in ICFR that is less severe than a material weakness, yet important enough to merit attention by those responsible for oversight of the company's financial reporting." A material weakness (MW) is defined as "a deficiency, or combination of deficiencies in ICFR such that there is a reasonable possibility that a material misstatement of the company's annual or interim financial statements will not be prevented or detected on a timely basis."

[5] AS No. 5 discusses circumstances under which control deficiencies are serious enough to be classified as significant deficiencies and material weaknesses. This new standard discusses deficiencies in controls, but does not explicitly refer to them as control deficiencies. However, the concept of a control deficiency is still implicit in AS No. 5.

[6] While AS No. 5 continues to require the auditor to make an independent assessment, this standard stresses the need for auditors to be more efficient and to allow management to perform more of the internal control testing.

Kennedy (1995) notes that the curse of knowledge phenomenon has been demonstrated in a number of audit contexts (e.g., Biggs and Wild 1985; Kinney and Uecker 1982; McDaniel and Kinney 1995). McDaniel and Kinney's (1995) study finds that auditors were unable to ignore management-provided summary data (i.e., book values) when developing expectations of account balances as part of their preliminary analytical procedures. They found that auditors who did not receive management's book values were more effective in identifying misstatements than auditors who were given management's book values prior to making their judgments.

As discussed earlier, in the ICFR setting auditors usually have management's classification available to them before they make their independent evaluation of the ICFR problem. In ICFR assessment task it is likely that auditors will be particularly prone to the curse of knowledge effect, because (unlike the case of unaudited book values) unless the ICFR problem actually results in a financial statement misstatement, auditors might never receive any feedback about the true outcome. Indeed, based on interviews with audit partners, Kinney et al. (2008) report that auditors find identification of ICFR design problems to be a difficult task when no misstatements have been detected. Therefore, we predict that in this unstructured, uncertain task, auditors will not be able to ignore how management classified the deficiency.

To the extent that management accurately classifies the company's ICFR problems, the curse of knowledge would not have a negative impact on auditors' subsequent testing and evaluation. However, there are at least two reasons to believe that management's assessments could be inaccurate and that auditors, therefore, should not rely on them. First, because prior to SOX, management was not required to evaluate the design of internal controls and to perform tests to evaluate their operating effectiveness, management is unlikely to be proficient at this task. Second, because management has an incentive to avoid the material weakness classification (as well as the subsequent adverse opinion on ICFR and the costs associated with remediation of the material weakness), management has an incentive to behave opportunistically, downplaying the severity of any internal control problems detected. Therefore, management classifications of problems might be self-serving (consciously or unconsciously).

Given the likelihood that the classification information that management provides is inaccurate and/or biased, auditors should refrain from using it and should arrive at classification judgments that are independent of management's. However, because the curse of knowledge is a hard-wired (unconscious) cognitive effect, auditors are likely to be influenced by management's classification, despite their attempts to be skeptical. We propose the following hypothesis:

> **H1:** Auditors' classifications of ICFR problems will be influenced by having management's initial classification at the time they make their judgments versus not having management's classification. Moreover, auditors' classifications will differ when they receive different management classifications.

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[7] Kennedy notes that the curse of knowledge can also occur is when one is evaluating other judges, and is unable to ignore information that was not available to the original judge at the time of the initial decision (Camerer et al. 1989).

Auditor as an Economic Agent

Despite the PCAOB's expectations that auditors arrive at their ICFR classifications independently, some researchers argue that because auditors are economic agents (e.g., Antle 1982), they might believe that there is some useful information in management's assessment of its internal controls. This agency theoretic/economic view suggests that auditors should consciously utilize management's information because they believe they will be more likely to make an accurate assessment if they do so. However, many articles have been written in the popular press about the audit environment since the passage of the Sarbanes-Oxley Act, and the importance of not being swayed by management. For example, Jim Cook of Ernst & Young, LLP suggests that there is now a "heightened importance of professional skepticism, audit quality, and diligent execution" (Barron 2005). The recent accounting and auditing scandals have sensitized auditors to criticisms that they have been too easily influenced by management's preferences and must restore their tarnished reputations (e.g., Frieswick 2003). Also, in a recent white paper issued by the CEOs of the six largest public accounting networks, the issue of professional skepticism was once again emphasized (International Audit Networks 2006).

To the extent that the auditor consciously uses management's classification as information (audit evidence), a rational auditor should still be professionally skeptical and consider management's incentives in classifying the ICFR problem. That is, a skeptical auditor should not use (or should place minimal reliance on) the information when management's classification might be self-serving. For example, in a case where the ICFR problem is at the margin between a significant deficiency and a material weakness, a rational auditor should place less reliance on management's assessment when management classifies this problem as a significant deficiency than as a material weakness. For at-margin cases between a significant deficiency and a material weakness, it is in management's interest to avoid the adverse opinion that would result from the more severe classification.

Kaplan et al. (2008) examined the effect of management's perceived incentives on judgments of the reliability of controls over a company's e-commerce sales system. They found that providing auditors with management's reliability rating of an e-commerce sales system influenced auditors' rating of that system. However, the incentives of management interacted with experience in that auditors with fewer years of experience were influenced by management's ratings when those ratings were favorable, whereas auditors with more experience were not influenced by management's ratings. While their study was conducted prior to SOX Section 404 testwork and the implementation of AS No. 2, and did not deal with ICFR problems or management classifications of these problems, Kaplan et al. (2008) conjecture that their results would have implications for a setting such as ours where management classifies ICFR problems before auditors perform their independent assessments of these problems.

In our setting, if auditors intentionally utilize management's information, then they should utilize management's classification of an ICFR problem to a lesser extent when that classification is less severe (i.e., more favorable to management) than when the classification is more severe. Thus, we hypothesize:

> **H2:** Auditors' assessments of an ICFR problem will be less influenced by management's classification when that classification is favorable to management (i.e., a less severe classification) than when the classification is unfavorable to management (i.e., a more severe classification).

Interestingly, it is theoretically possible for both H1 and H2 to hold simultaneously. Hobson and Kachelmeier (2005) discuss how a cognitive bias and an intentional discounting of information could operate at the same time. Hobson and Kachelmeier (2005) investigated a setting where they varied whether sellers were given the option of choosing the information to disclose to buyers (i.e., high or low outcome related to a risky prospect) or were constrained to disclose information randomly. The results indicated that buyers understood the sellers' incentives to disclose information that was favorable to them (i.e., to disclose the high outcome), and that buyers discounted the information disclosed in this situation. However, the buyers still fell prey to the cognitive effect of unconsciously utilizing the information and having it affect their judgments, even when they had economic incentives to avoid using the information. Hobson and Kachelmeier (2005) showed that both the unintentional cognitive effect and the intentional discounting of biased information occurred simultaneously, and were independent of each other (i.e., the two effects did not interact).

Reducing the Bias through a Documentation Requirement

Prior research (Kennedy 1995) suggests that cognitive biases such as the curse of knowledge could be remedied through an intervention that involves cognitive restructuring of the task. Therefore, we examine whether the curse of knowledge bias can be mitigated by an intervention that asks auditors who have received management's classification to consider the impact of the ICFR problem on the financial statements. The PCAOB standards' guidance for classifying ICFR problems requires auditors to determine whether each problem has the potential to result in a financial statement misstatement, the likelihood of such a misstatement occurring, and the possible magnitude of the misstatement (AS No. 2, paragraph 131 and AS No. 5, Appendix A). In addition, AS No. 3 (paragraph 12) requires the auditor to document significant findings and issues, including "the existence of significant deficiencies or material weaknesses over financial reporting" (AS No. 3, paragraph 12). However, none of the standards explicitly requires the auditor to document the likelihood or magnitude information associated with the identified ICFR problems, nor the resulting impact of an identified problem on the financial statements. Because ICFR problems have the *potential* to result in misstatement, but misstatement has not always occurred, such judgments are very subjective in nature and may require the auditor to develop hypothetical outcomes (i.e., the resulting financial statement impacts) for each of the identified ICFR problems.[8]

Psychology research (e.g., Renkl 1997) suggests that developing and documenting these hypothetical outcomes leads to cognitive restructuring of the task, which could reduce the curse of knowledge bias when evaluating management's classifications. To the extent that auditors spontaneously develop hypothetical outcomes without explicitly being instructed to do so, instructions to document how an ICFR problem can lead to a financial statement misstatement would not have any influence on their judgments. However, if they do not explicitly consider the specific ways that an ICFR problem could lead to a financial statement misstatement, instructions to reason this way (and to document this reasoning) could help reduce any curse of knowledge effects that are present.

Studies in cognitive psychology and auditing have found that requiring individuals to provide explanations while solving ill-structured tasks can improve their performance of the task (Renkl 1997; Renkl et al. 1998; Earley 2001, 2003). Earley (2001, 2003) found that when auditors generated explanations for why management-provided information was or was not reasonable, they improved their subsequent audit judgments. Similarly, Ashton and Kennedy (2002) found that requiring auditors to engage in "self-review" has been shown to mitigate certain influences on judgment. Presumably, the improvement gained from "self-review" occurs because developing explanations requires the individuals to refine their mental models of the situation.

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[8] As noted in AS No. 2 (paragraph 132), "The significance of a deficiency in internal control over financial reporting depends on the *potential* for a misstatement, not on whether a potential misstatement actually has occurred" (emphasis in original).

In the context of the internal control task, we expect an explicit requirement to document how an ICFR problem can lead to a financial statement misstatement to mitigate the curse of knowledge effects predicted in H1. Prior to self-explaining, the auditor is expected to be influenced by management's classification of the problem. When auditors engage in the process of documenting the possible impact of the ICFR problem on the financial statements, they should be able to think about the task differently and develop a better understanding of how the control weakness might ultimately impact the financial reporting process.[9] As a result, judgments made after this documentation process are expected to be based more on how the ICFR problem will impact the financial statements, and not as much on management's classification of the problem, as hypothesized below:

> **H3:** Auditors who are required to document the possible financial statement impact of an ICFR problem will provide judgments after performing this documentation that are less consistent with management's classification of the problem than their judgments prior to performing this documentation.

To test the hypotheses, two experiments were conducted, and are described in the following two sections.

III. EXPERIMENT 1: TEST OF MANAGEMENT'S FIRST MOVER INFLUENCE ON AUDITORS' JUDGMENTS

Method

Participants

Participants for Experiment 1 were 97 in-charge auditors from a Big 4 public accounting firm who had a mean level of experience of 4 years and a range of 2 to 12 years of experience. The participants were attending a firm-wide training program. There was no difference in experience level across treatment conditions and the participants were randomly assigned to treatment conditions. In-charge auditors routinely perform the task of evaluating ICFR problems, which is the focus of this study.[10] At the time that the experiment was conducted auditors had completed at least one year of SOX 404 audit procedures for their listed clients. Participants in this study had a mean of 418 hours of SOX 404 engagement experience and there was no difference in SOX experience across treatment groups.

Design

Experiment 1 was designed to test H1 and H2. Specifically, H1 asks whether auditors' initial assessments of ICFR problems (before the design evaluation stage of the process) are influenced by having management's classification at the time they make their judgments versus not having management's classification. Hypothesis 1 also explores whether any such influence differs based on the nature of management's classification (e.g., whether auditors reach different judgments when management's classification is a significant deficiency versus a material weakness). Hypothesis 2 investigates whether auditors intentionally use management's information, discounting classifications that are more favorable to management, such that auditors are less influenced by management's less severe classifications than by their more severe classifications. We employed a 3 × 6 experimental design, which had one between-participants variable and one within-participants variable. The between-participants variable was Management Classification, which was varied at three levels (Classification Not Provided, Classification Set 1, and Classification Set 2). The within-participants variable was Case, and was varied at six levels (Case 1 through Case 6).[11] The dependent variable was an assessment of the ICFR problem on an 11-point scale, where 1 = Absolutely a control deficiency, 6 = Absolutely a significant deficiency, and 11 = Absolutely a material weakness.

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[9] Note that other auditing researchers (e.g., Koonce 1992; Anderson and Koonce 1995) have examined the "explanation effect," which is different from the "self-explanation effect" in the cognitive learning literature. In explanation effect studies, the auditor is asked to explain how an outcome could occur. Explaining how an outcome can occur makes it appear more likely that it will occur (and counterexplaining how it might not occur makes it seem less likely to occur). Self-explaining how the process works is different in that it is not designed to change the probability of the outcome occurring. Rather, it is an approach that makes it more likely that the auditor will attend more deeply to the evidence and not be influenced as much by other cognitive factors, such as management's classification.

[10] Managers and partners will likely perform the final internal control assessments to determine whether there are any items that must be reported (i.e. material weaknesses resulting in an adverse opinion on internal controls). However, we are investigating the initial assessments made during planning, which are often made by in-charge auditors. In addition, previous research by Ricchiute (1999) has suggested that higher-level auditors are influenced by the recommendations made by their subordinates.

In all treatment conditions, each participant received a description of an ICFR problem found at each of six independent clients. The cases were designed so that they were on the margin between two categories. That is, some cases were designed to be less severe and at the margin between a control deficiency and a significant deficiency (i.e., CD/SD cases), and some cases were designed to be more severe and at the margin between a significant deficiency and a material weakness (i.e., SD/MW cases). For each identical case, half of the participants received a more severe management classification (e.g., a material weakness) and the other half of the participants received a less severe management classification (e.g., a significant deficiency). The order in which participants were provided with the more severe versus less severe management classification was varied for each case. These treatment conditions were labeled "Classification Set 1" and "Classification Set 2," to indicate the order of management classifications provided for each set of cases. The design of Experiment 1 is presented in Table 1.

In the control condition (i.e., the Classification Not Provided condition), participants were not provided with management's classification of the ICFR problem. This condition enables us to determine how severe auditors perceived the problem described in each case to be without being influenced by management's classification of that problem.

Except for modifications related to each condition as described above, all participants in Experiment 1 received the same case materials. All of the clients were identified as medium-sized companies of low audit risk that had been in operation for more than 25 years. As background, participants were advised that they were the lead senior auditor on each of the client cases that would be presented and that they should consider each of the client cases independently from the others. Participants were informed that they were making preliminary control weakness assessments as part of the SOX 404 testwork component of the integrated audit. They were also informed that this initial ICFR assessment takes place before the design evaluation stage and that their audit firm's walkthroughs and tests of the design and operating effectiveness of key controls had not yet been completed.

Participants then reviewed the description of the ICFR problem for each of six client companies. We adapted the six client cases from actual companies who filed SOX internal control reports and remedial actions with the SEC. To ensure that the companies were not recognizable, we altered their names and any of their identifiable features.[12] For each case, we varied the severity of

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[11] We also included a second between-participants variable, Advice, which varied whether or not management had obtained assistance from an external advisor (i.e., another CPA firm) in making their classifications of the internal control deficiencies. There was no significant main or interactive effect of the Advice variable on participants' internal control assessments. Accordingly, we collapse across the Advice variable when reporting the results of our analyses.

[12] In order to determine whether the cases were realistic and that the deficiencies were subtle enough to require some judgment in classification, we consulted with two expert SOX 404 auditors who provided us with valuable feedback which aided us in the development of the cases. We also pilot tested the cases on 12 auditors at the professional rank and experience level of participants in this study to provide further validation of the instrument.

TABLE 1

Design of Experiment 1
Test of the Influence of Management's ICFR Classification on Auditors' ICFR Assessments

Case (Manipulated within-participant)	Management Classification Condition (manipulated between-participants): Classification Not Provided	Classification Set 1—Mgmt. Classified ICFR as:	Classification Set 2—Mgmt. Classified ICFR as:
Case 1—Raven (Fixed Assets) At-margin case between SD and MW	Not Provided	MW	SD
Case 2—White (Revenue and Accts. Rec.) At-margin case between SD and MW	Not Provided	SD	MW
Case 3—Harper (Revenue) At-margin case between CD and SD	Not Provided	CD	SD
Case 4—Konrath (Revenue and Inventory) At-margin case between SD and MW	Not Provided	MW	SD
Case 5—Alexander (Fixed Assets and Depreciation) At-margin case between CD and SD	Not Provided	SD	CD
Case 6—Ballas (Inventory) At-margin case between SD and MW	Not Provided	SD	MW

Participants were provided with a case that was at the margin of two ICFR classifications. For a description of the case facts, see the Appendix. The three possible ICFR classifications are Control Deficiency (CD), Significant Deficiency (SD), and Material Weakness (MW). Of the six cases provided to participants, two were less severe (i.e., at the margin of a control deficiency and a significant deficiency) and four were more severe (i.e., at the margin of a significant deficiency and a material weakness). In the Classification Not Provided condition, participants were not provided with management's classification of the ICFR problem. In the other two Management Classification treatment conditions, half of the participants were provided with the less severe management classification and the other half were provided with the more severe management classification. In Classification Sets 1 and 2, we counterbalanced the severity of management's classifications. Case 2 was a failed manipulation because the case was perceived by participants to be a less severe case (like Cases 3 and 5), but the cue provided was that of a more severe case. We discuss in the text why we believe this case was perceived differently from our intention.

the ICFR problem by varying the nature of the information provided regarding the likelihood and magnitude of a material misstatement resulting from each deficiency. Because an adverse opinion must be issued when the auditor determines that a control deficiency has reached the level of a material weakness, the cases at the margin between a significant deficiency and a material weakness have the highest stakes. Therefore, we included four of these cases (i.e., SD/MW cases). We also included two cases at the margin between a control deficiency and a significant deficiency (i.e., CD/SD cases) for two reasons. First, we wanted the cases to differ in their severity so participants would have to attend carefully to each case and not just assume they were all similar. Also, classifying ICFR problems as significant deficiencies is important because of their potential to rise to the level of a material weakness if there are enough of them in the aggregate. The Appendix includes excerpts from each of the six cases.

To recap, we used four cases that were intended to be more severe (i.e., SD/MW cases) and two cases that were intended to be less severe (i.e., CD/SD cases). We then varied the cue presented to participants as management's classification of the ICFR problem to be on the margin between the two classifications. For example, Cases 3 and 5 were designed to be at the margin between a control deficiency (the less severe classification) and a significant deficiency (the more severe classification). For these two CD/SD cases, half of the participants were told that management had classified the problem in the case as a control deficiency (CD) and the other half were told that the same problem had been classified by management as a significant deficiency (SD). We counterbalanced the order of presentation of management's classifications. Similarly, for the remaining four of the six cases (i.e., Cases 1, 2, 4, and 6), the severity of the deficiency was designed to be on the margin between a significant deficiency (SD) and a material weakness (MW). Half of the participants were told that management classified the problem as a significant deficiency (SD) and the other half were told that it was classified as a material weakness (MW).

Participants were instructed to refrain from advancing ahead or going back to change their responses and were instructed not to talk with other participants during the study. The administrators of the experiment monitored the participants to ensure adherence to the instructions. The final phase of the experiment was the completion of the background questionnaire, which included questions about participants' background and work experience.

Results of Experiment 1

We performed a manipulation check asking participants to indicate whether management's classification of the ICFR problem had been provided to them before they provided their own assessments. Results indicate that 92.8 percent of participants correctly indicated whether the management classification was or was not provided, and there was no difference in accuracy between treatment groups.[13]

To address H1, we ran a 3×6 repeated measures ANOVA where the between-participants variable, Management Classification, had three levels (Classification Not Provided, Classification Set 1, and Classification Set 2) and the within-participants variable, Case, had six levels (Case 1 through Case 6). There was a significant interaction between Management Classification and Case ($F = 3.70$, $p < 0.001$), indicating that the effect of Management Classification differed by case. This interaction makes sense because the cases themselves differed on two dimensions. That is, the situations described in the cases differed in their severity (i.e., the case was designed to either be at the margin between CD/SD or between SD/MW) and in the specific classification cue received (whether the participants received a CD, SD, or MW or were not provided with a classification at all). Therefore, to address our specific hypotheses regarding the particular classification cue received (Management Classification), we conduct separate tests, described below, for each of the two severities of cases (CD/SD and SD/MW). This information is presented in Table 2.

The Classification Not Provided condition provides evidence regarding participants' perceptions of the severity of the ICFR problem described in each case independent of management's classification. The results indicate that participants did not perceive one of the cases as we had designed it to be perceived. Specifically, in the Classification Not Provided condition, Case 2 (which was designed to be an SD/MW case) was perceived by participants to be more like the

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[13] We included all participants in our analyses. To ensure that including the seven participants who failed to answer this question correctly did not affect our results, we also conducted the analyses excluding these participants, and found that the results were essentially the same regardless of whether these seven participants were included.

TABLE 2

Results of Experiment 1

Auditors' Assessments of ICFR Problems and Curse of Knowledge Effects by Management Classification Condition

Panel A: Mean Auditor ICFR Assessments (Standard Deviations) for Cases Designed to be Less Severe (at Margin between Control Deficiency and Significant Deficiency)

	Cell Means by Management Classification Condition^{a,b}			Differences Due to Management's ICFR Classification^{c} ("Curse of Knowledge")		
Case	Classification Not Provided	Classified Lower (CD Classification)^{c}	Classified Higher (SD Classification)^{d}	Classification Not Provided minus CD Classification	SD Classification minus Not Provided	SD Classification minus CD Classification
Case 3—Harper (Revenue) At-margin case between CD and SD	5.96 (3.07) n = 28	4.60 (2.60) n = 35	6.30 (2.95) n = 33	1.36	0.34	1.70
Case 5—Alexander (Fixed Assets and Depreciation) At-margin case between CD and SD	5.18 (2.51) n = 28	4.33 (2.53) n = 33	5.57 (2.54) n = 35	0.85	0.39	1.24
Marginal Means^{d}	5.57	4.47	5.94	1.10 t = 2.36 p = 0.01	0.37 t = 0.72 p = 0.24	1.47 t = 3.34 p = < 0.001

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TABLE 2 (continued)

Panel B: Mean Auditor ICFR Assessments (Standard Deviations) for Cases Designed to be More Severe (at Margin between Significant Deficiency and Material Weakness)

	Cell Means by Management Classification Condition[a,b]			Differences Due to Management's ICFR Classification[c] ("Curse of Knowledge")		
Case	**Classification Not Provided**	**Classified Lower (SD Classification)[c]**	**Classified Higher (MW Classification)[d]**	**Classification Not Provided minus SD Classification**	**MW Classification minus Not Provided**	**MW Classification minus SD Classification**
Case 1—Raven (Fixed Assets) At-margin case between SD and MW	8.29 (2.32) n = 28	6.79 (2.64) n = 33	8.09 (2.02) n = 35	1.50	−0.20	1.30
Case 4—Konrath (Revenue and Inventory) At-margin case between SD and MW	8.57 (2.41) n = 28	7.70 (2.48) n = 33	8.31 (2.77) n = 35	0.87	−0.26	0.61
Case 6—Ballas (Inventory) At-margin case between SD and MW	7.14 (3.03) n = 28	7.37 (2.88) n = 35	8.55 (2.82) n = 33	−0.23	1.41	1.18
Marginal Means[e]	8.00	7.29	8.32	0.71 t = 1.87 p = 0.03	0.32 t = 0.68 p = 0.25	1.03 t = 2.73 p = < 0.001

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[a] Dependent Variable: Mean assessment of the ICFR problem on an 11-point scale, where 1 = Absolutely a Control Deficiency (CD), 6 = Absolutely a Significant Deficiency (SD), and 11 = Absolutely a Material Weakness (MW).

[b] In the Classification Not Provided condition, participants were not provided with management's classification of the ICFR problem. In Panel A, the Cases were less severe cases at the margin of Control Deficiency (CD) and Significant Deficiency (SD), so the Classified Lower condition participants were told that management classified the ICFR problem as a Control Deficiency and the Classified Higher condition participants were told that management classified the ICFR problem as a Significant Deficiency. In Panel B, the Cases were more severe cases at the margin of Significant Deficiency (SD) and Material Weakness (MW), so the Classified Lower condition participants were told that management classified the ICFR problem as a Significant Deficiency and the Classified Higher condition participants were told that management classified the ICFR problem as a Material Weakness.

(continued on next page)

TABLE 2 (continued)

[c] The first two columns measure the influence on auditors' judgments of being provided with management's classification of the ICFR problem (i.e., having management's classification) before making their own assessments. Since auditors usually are provided with management's classifications before making their own assessments, the third column is important because it measures how the nature of management's classification of the ICFR problem (i.e., whether management classifies the ICFR problem as being of lower or higher severity) influences auditors' assessments.

[d] In the 2 × 3 ANOVA, Management Classification condition was significant ($F = 5.25$, $p < 0.01$); Case was not significant ($F = 2.62$, $p = 0.11$); and the interaction of Management Classification condition × Case was not significant ($F = 0.16$, $p = 0.86$). Because there was no interaction, statistical analyses are performed on the marginal means.

[e] In the 3 × 3 ANOVA, Management Classification condition was significant ($F = 4.04$, $p = 0.02$); Case was not significant ($F = 1.13$, $p = 0.35$); and the interaction of Management Classification condition × Case was not significant ($F = 1.11$, $p = 0.36$). Because there was no interaction, statistical analyses are performed on the marginal means.

CD/SD cases (Cases 3 and 5) than like the SD/MW cases (Cases 1, 4, and 6).[14] Given that Case 2 was not perceived to be at the margin of SD/MW, the cues we used as management's classification for this case in the treatment groups were not appropriate. Therefore, we consider this case to be a failed manipulation, and exclude it from further analysis.

To test H1 and H2, we conducted one test for the less severe cases (Cases 3 and 5), which were at the margin between CD and SD. We conducted a separate test for the more severe cases (Cases 1, 4, and 6), which were at the margin between SD and MW. For Cases 3 and 5 (the CD/SD cases), participants had three possible treatment conditions: (1) they were not given a prior classification by management (Classification Not Provided), (2) they were told that management classified the ICFR problem as a control deficiency (CD Classification), or (3) they were told that management classified the problem as a significant deficiency (SD Classification). Panel A of Table 2 presents the mean assessments and standard deviations for the less severe cases (Cases 3 and 5).

To test H1 for Cases 3 and 5, we ran a 2 × 3 ANOVA for the two cases by three classifications. The interaction between Management Classification and Case was not significant ($F = 0.16$, $p = 0.86$), and the main effect of Case was not significant ($F = 2.62$, $p = 0.11$), indicating that Cases 3 and 5 were similar to each other. The main effect of Management Classification was significant ($F = 5.25$, $p < 0.01$), indicating that management's classification of the ICFR problem influenced judgments, consistent with an overall curse of knowledge effect.[15] Hypothesis 1 predicted that having management's classification would influence auditors' judgments compared to not having that classification. To test this part of H1, we compared each treatment condition to the Classification Not Provided condition. We found that the CD Classification condition (mean = 4.47) resulted in lower judgments ($t = 2.36$, $p = 0.01$, one-tailed) than in the Classification Not Provided condition (mean = 5.57). The SD Classification condition (mean = 5.94) was directionally higher than the Classification Not Provided condition, although this difference was not statistically significant ($t = 0.72$, $p = 0.24$, one-tailed). Hypothesis 1 also predicted that auditors' classifications would differ when they received different management classifications. To test this part of H1, we compared the two treatment conditions to each other. Participants who received the CD Classification (mean = 4.47) responded with judgments that were significantly lower than ($t = 3.34$, $p < 0.001$, one-tailed) those who received the SD Classification (mean = 5.94), indicating that participants were influenced by management's classification. Thus, overall, results were consistent with H1 in that there was a curse of knowledge effect. Participants were influenced by management's classifications differentially when they received the CD Classification versus SD Classification. Compared to not receiving any classification by management, the effect was statistically significant from Classification Not Provided treatment group only for the CD Classification condition, but not for the SD Classification condition. Note that participants were more influenced by management's classification when management's classification was less severe and was favorable to management (i.e., CD Classification condition) than when the classification was more severe and was unfavorable to management (i.e., SD Classification condition). This result is not consistent with H2, because it is inconsistent with the auditors using the information as rational economic agents and discounting the information that was favorable to management.

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[14] Mean assessments for Case 2 were 6.25 in the Classification Not Provided condition, 7.46 in the SD Classification condition, and 7.76 in the MW Classification condition. Results of a one-way ANOVA with specified *a priori* contrasts indicated that participants in the Classification Not Provided condition perceived Case 2 to be different from Cases 1, 4, and 6 ($t = 3.06$, $p < 0.001$), but not to be different from Cases 3 and 5 ($t = 1.06$, $p = 0.29$). One possible explanation for this is that Case 2 is similar to Cases 3 and 5 in that each of these three cases does not identify the amount of any potential misstatement that could occur, whereas Cases 1, 4, and 6 all mention the amount of misstatement that could occur. It is possible that when no dollar amount of potential misstatement is explicitly mentioned, the participants perceived the case to be less severe.

[15] The results reported here (and in the analysis for the more severe cases) treat Case as a within-participant variable. We also conducted an ANOVA that treated Case as a between-participant variable and obtained virtually identical results.

We conducted a similar analysis to test H1 and H2 for Cases 1, 4, and 6, which were on the margin between SD/MW. Panel B of Table 2 presents the mean assessments and standard deviations for this analysis. We ran a 3 × 3 ANOVA for the three cases (1, 4, and 6) by three classification conditions (Classification Not Provided, SD Classification, or MW Classification). The interaction between Management Classification and Case was not significant ($F = 1.11$, $p = 0.36$), and the main effect of Case was not significant ($F = 1.13$, $p = 0.35$), indicating that Cases 1, 4, and 6 were similar to each other. The main effect of Management Classification was significant ($F = 4.04$, $p = 0.02$), indicating that management's classification influenced judgments, consistent with an overall curse of knowledge effect.

To test the part of H1 that predicted that having management's classification would influence auditors' judgments compared to not having that classification, we compared each treatment condition to the Classification Not Provided condition. The SD Classification condition (mean = 7.29) resulted in lower judgments ($t = 1.87$, $p = 0.03$, one-tailed) than in the Classification Not Provided condition (mean = 8.00), but the MW Classification condition (mean = 8.32) was directionally higher than the Classification Not Provided condition, although this difference was not statistically significant ($t = 0.68$, $p = 0.25$, one-tailed). To test the part of H1 that predicted that auditors' classifications would differ when they received different management classifications, we compared the two treatment conditions to each other.

When presented with a case at the margin between SD/MW, participants who received the SD Classification (mean = 7.29) responded with judgments that were significantly lower than ($t = 2.73$, $p < 0.001$, one-tailed) those who received the MW Classification (mean = 8.32), indicating that participants were influenced by management's classification. Thus, overall, the results were consistent with H1 in that there was a curse of knowledge effect, and the results were quite similar to the analysis for the less severe cases (Cases 3 and 5). Specifically, participants were influenced by management's classifications differentially when given SD Classification versus MW Classification. Compared to not receiving any classification from management, the effect was statistically significant from the Classification Not Provided condition only for the SD Classification condition, not for the MW Classification condition. Also similar to the results for the less severe cases, participants were more influenced by management's classification when management's classification was less severe and was favorable to management (i.e., SD Classification condition) than when the classification was more severe and was unfavorable to management (i.e., MW Classification condition), which is not consistent with H2.

Experiment 2 was conducted to test whether asking participants to document the financial statement impact of the ICFR problems would help them to restructure the task and mitigate the curse of knowledge effects found in Experiment 1.

IV. EXPERIMENT 2: REDUCING THE CURSE OF KNOWLEDGE ON AUDITORS' JUDGMENTS

Method

Participants

Participants for Experiment 2 were 25 in-charge auditors from the same Big 4 accounting firm attending the same national training session as the participants in Experiment 1, but this experiment was administered in a separate room. Experiment 2 participants had a mean of 4 years and a range of 2 to 10 years of experience in public accounting. The mean level of SOX 404 engagement experience was 432 hours. One participant failed to complete the instrument, and was therefore excluded from further analysis, resulting in 24 usable responses.

Design

Experiment 2 was designed to test H3. In this experiment, participants were given a subset of the cases provided to participants in the first experiment. The participants were provided with management's classification of the ICFR problem prior to providing their own assessments and classifications. All three types of ICFR problems (CD, SD, and MW) were provided to auditors as cues in Experiment 2.[16]

Procedure

Participants were first presented with the same initial background information that was presented in Experiment 1. They were advised that they were the lead senior auditor on each of the client cases and that they should consider each case independently of the others. Participants were informed that they were making preliminary judgments of ICFR problems as part of the SOX 404 testwork component of the integrated audit. They were also informed that the audit firm's walkthroughs and tests of design and operating effectiveness of key controls had not yet been completed.

After reviewing the description of each client's ICFR problem, participants made an initial assessment of the severity of the problem on an 11-point scale, where 1 = Absolutely a control deficiency, 6 = Absolutely a significant deficiency and 11 = Absolutely a material weakness. After making the initial assessment, participants placed their responses in an envelope (Envelope 1) and opened a second envelope (Envelope 2). The second stage of the study instructed the participants to consider the potential financial statement impact of each ICFR problem if the problem was not discovered or remediated. Participants were asked to document their assessments by identifying a specific financial statement account that would be affected and the nature of the effect, regardless of its materiality.[17] They were then asked to reassess the severity of the ICFR problem on an 11-point scale, as described above.

As in Experiment 1, participants were instructed to refrain from advancing ahead or going back to change their responses and were instructed not to talk with other participants during the study. The administrators of the experiment monitored the participants to ensure adherence to the instructions. The final phase of the experiment was the completion of the background questionnaire, which included questions about participants' background and work experience.

Results of Experiment 2

Hypothesis 3 addressed whether instructing participants to document the possible financial statement impact of the ICFR problems would mitigate any curse of knowledge bias resulting from having been provided with management's classification of the problems prior to performing their own assessments. In order to test this hypothesis, we examined the difference in the mean assessments from pretest to posttest for each of the three cases. The mean assessments (standard deviations) for each of the management classifications of the ICFR problems (the initial cues provided to participants) are presented in Table 3.

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[16] In Experiment 2, we used four of the cases from Experiment 1 (Harper, Ballas, Raven, and White). However, as discussed in the results section of Experiment 1, the White case resulted in a failed manipulation and was dropped from the study.

[17] The experimental manipulation required participants to assess the possible financial statement impact of each ICFR problem by itself, so they were not provided with the likelihood and magnitude information for each case, as were participants in Experiment 1. Because participants were being asked to develop their own assessments of financial statement impact (i.e., likelihood and magnitude), this information was deliberately left vague. Participants were told that there was at least a moderate possibility of misstatement resulting from each of the ICFR problems. Due to differences in information provided to participants between the two experiments, it is not appropriate to make direct comparisons between the results of Experiment 1 and Experiment 2.

TABLE 3

Results of Experiment 2

The Effects of Requiring Auditors to Document Potential Financial Statement Impact of ICFR Problems on the Curse of Knowledge

Case[a]	Management Classification of the ICFR Problem Provided to Auditors Prior to Documentation	Auditor's Mean Assessment of ICFR Problem *before* Documentation (Std. Dev.)[b]	Auditor's Mean Assessment of ICFR Problem *after* Documentation (Std. Dev.)	Difference in Auditors' Mean Assessments before and after Documentation (Std. Dev.)	t-stat.	p-value (one-tailed)
Case 3[c]—Harper (Revenue) At-margin case between CD and SD	CD	4.13 (2.46) n = 24	5.83 (3.21) n = 24	−1.70 (2.62)	3.18	<0.01
Case 6—Ballas (Inventory) At-margin case between SD and MW	SD	6.38 (3.46) n = 24	7.25 (2.51) n = 24	−0.87 (2.44)	1.76	0.05
Case 1—Raven (Fixed Assets) At-margin case between SD and MW	MW	7.83 (2.87) n = 24	7.13 (2.44) n = 24	0.70 (2.27)	1.53	0.07

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[a] In Experiment 2, we used four of the cases from Experiment 1 (Harper, Ballas, Raven, and White). However, as discussed in the results section of Experiment 1, the White case resulted in a failed manipulation and was dropped from the study. Due to differences in information provided to participants between the two experiments, it is not possible to make direct comparisons between the results of Experiment 1 and Experiment 2. In Experiment 2, we did not provide participants with the likelihood and magnitude information that was provided to participants in Experiment 1 because the experimental manipulation required them to develop their own assessments of the potential financial statement impact (i.e., likelihood and magnitude).

[b] Dependent Variable: Mean assessment of the ICFR problem on an 11-point scale, where 1 = Absolutely a Control Deficiency (CD), 6 = Absolutely a Significant Deficiency (SD), and 11 = Absolutely a Material Weakness (MW).

[c] We present the three cases in the order we discuss them in the text, which is from the least severe management classification (Management Classified ICFR problem as a Control Deficiency) to the most severe classification (Management Classified ICFR problem as a Material Weakness).

To guard against possible demand effects inherent in using a within-participant design, we designed the cases so that mitigating the curse of knowledge bias would sometimes require an increase in judgments from pretest to posttest, and would sometimes require a decrease in judgments from pretest to posttest. Varying the direction of the change in judgment prevented participants from "getting the correct answer" by always making their judgments in the same direction. Because the responses for the cases would have to move in different directions to demonstrate a reduction in the curse of knowledge bias, we conducted separate t-tests for each case. The Harper Case (adapted from Case 3 of Experiment 1) was designed to be at the margin between CD/SD, and participants received the management's classification of CD (i.e., CD Classification). Therefore, we would expect participants' initial assessments to be closer to CD than to SD before they performed the documentation task. The pre-documentation mean assessment was 4.13, which is relatively less severe on the 11-point scale and is consistent with management's CD classification. After participants documented the potential financial statement impact, their mean assessment increased ($t = 3.18$, $p < 0.01$, one-tailed) to 5.83, which is closer to SD.

The Ballas case (adapted from Case 6 of Experiment 1) was designed to be at the margin between SD/MW, and participants were provided with management's classification of SD (i.e., SD Classification). Therefore, we would expect participants' initial assessments to be closer to SD than to MW before they performed the documentation task. The pre-documentation mean assessment was 6.38, which is consistent with management's SD cue. After documentation, the assessment moved toward the MW classification. That is, the post-test mean assessment was 7.25, which was significantly higher than the pretest assessment ($t = 1.76$, $p = 0.05$, one-tailed).

The Raven Case (adapted from Case 1 of Experiment 1) was also designed to be at the margin between SD/MW, but in this case participants were provided with management's classification of MW (i.e., MW Classification). Prior to documenting, the mean assessment was 7.83, which is between the SD and MW classification. After documenting, the assessment decreased toward the SD classification to 7.13. For this case, the decrease in means was not statistically significant at conventional levels, but was significant at $\alpha = 0.07$ [$t = 1.53$, $p = 0.07$, one-tailed].

Overall, the results of Experiment 2 were consistent with H3, because in all three cases the posttest assessment after documentation moved away from the initial assessment indicated by management's cue.

V. DISCUSSION AND CONCLUSION

This study examined a situation in which management generates summary information that the auditor then assesses, rather than the auditor independently arriving at his/her own judgment. We investigate whether the summary information provided by management as the "first mover" influences auditors' judgments. The specific setting we studied was auditors' initial ICFR assessments under SOX 404b, where auditors receive management's classification of an ICFR problem before making their own classification judgment.

Our first experiment found that auditors exhibit the curse of knowledge bias in that their judgments of the severity of the ICFR problem differs based on management's classification of the problem. This violates PCAOB standards that require auditors to make an independent assessment of ICFR problems. This bias in auditors' initial judgments could reduce audit effectiveness because it is likely to influence which controls auditors focus on when performing walkthroughs of the client's system. It could also influence auditors' tests of controls and, in some cases, how much they believe they are able to rely on the work of others, such as internal auditors (AS No. 2, paragraphs 108–126, also AS No. 5 paragraphs 16–19). Therefore, it is important to try to find a remedy for the curse of knowledge effect in auditors' ICFR assessments.

Prior research has found that the curse of knowledge is a hard-wired cognitive effect (i.e., one that cannot be removed by effort or incentives) that is best remedied by cognitively restructuring the task. Therefore, our second experiment tested such an intervention. Specifically, we cognitively restructured the task by asking auditors to document the potential financial statement impact of each ICFR problem, assuming the problem was not discovered or remediated. Although the PCAOB auditing standards require the auditor to evaluate each identified ICFR problem in terms of its potential to result in a material misstatement to the financial statements and to reason through the eventual outcome of the problem, AS No. 2 and AS No. 5 do not require auditors to document their reasoning process. AS No. 3, the current documentation standard, also does not specifically require auditors to document how the ICFR problem would impact the financial statements. Our results indicate that our intervention requiring auditors to explicitly consider and document the nature of potential misstatements, and how they could occur, was successful in reducing the curse of knowledge bias.

Management's influence on auditors' ICFR assessments could be particularly problematic when management's initial classification is incorrect or is biased to downplay the severity of any observed problem. A surprising result was that in the post-SOX era, we found that auditors were more influenced by management's classification (i.e., the curse of knowledge bias was stronger) when management classified the ICFR problem as less severe, which is self-serving because it is more favorable to management. One would expect auditors to be more professionally skeptical in the current audit environment, and not to be more influenced in this situation.

While previous research in motivated reasoning (e.g., Kunda 1990; Hackenbrack and Nelson 1996; Salterio and Koonce 1997; Jenkins and Haynes 2003; Kadous et al. 2003; Blay 2005) has documented that auditors are subconsciously influenced to try to please management, these studies were conducted prior to SOX. Because the post-SOX era is associated with higher audit risk due to regulation and annual PCAOB inspections, one would expect auditors not to be as swayed by their clients as they might have been previously. Several prior studies (e.g., Blay 2005; Hackenbrack and Nelson 1996) found that when there was high risk, the effects of motivated reasoning were diminished or eliminated. While the current study was not designed to investigate motivated reasoning, because it did not focus on the negotiation process at the final stage of the internal control assessment process, one would expect that the risk of being too lenient with management in the current audit environment would seem to be prohibitively high. Future research could be specifically designed to test whether motivated reasoning is still prevalent in the post-SOX environment.

This study also has implications for other situations in which management is the "first mover" in reaching conclusions about information that impacts the audit. Auditors are now being required to test whether management's valuations and accounting estimates are reasonable, instead of reaching these types of audit judgments independent of management's judgment. For example, when making fair value judgments under SFAS No. 157 (FASB 2006), management has to categorize assets into one of three categories. Will auditors be inappropriately influenced by management's classifications in making their own judgments about fair value? The results of this study, combined with findings from (Earley 2002) regarding experienced auditors' fair value judgments, indicate that auditors could be biased by management's classifications. Given the prevalence of situations where the auditor is the "second mover," it is possible that remedies, such as giving auditors explicit instructions to document their reasoning, might be required in order to mitigate these biases.

There are several limitations to this study. For example, it is beyond the scope of this research to address how initial assessments and classifications of ICFR problems will ultimately impact the extent of tests of design and operating effectiveness of controls and the resulting audit opinions on the client's internal controls. This study is also subject to the limitations of all experimental settings, such as the inability of participants to interact with management and request additional information to aid them in their assessments. In addition, our experimental materials did not include any information on whether internal auditors assisted management in making their classifications of ICFR problems. However, in practice, some clients use their internal audit staff to test internal controls. AS No. 5 allows auditors to rely more on the work of internal auditors than AS No. 2 did, but it is an open question how this increased reliance affects auditors' independent ICFR judgments. These limitations notwithstanding, our study documents a simple way to remedy the negative influence of management-provided summary information and conclusions on auditors' judgments.

APPENDIX

Summary of Case Facts Provided to Participants

		Management Classification of the ICFR Problem (manipulated between-participant)	
Case	**Case Summary**	**Classification Set 1**	**Classification Set 2**
Case 1—Raven (Fixed Assets) At-margin case between a significant deficiency and a material weakness	Several fixed asset internal control weaknesses were detected including poor documentation of policies and procedures, failure to identify and locate fixed assets, and lack of usage of company identification tags. The control weaknesses identified resulted in a 2.4 percent overstatement in net income (arising from improper capitalization of repairs and maintenance expenses), which was detected and corrected before year-end.	Material Weakness	Significant Deficiency
Case 2—White (Revenues and Accounts Receivable) At-margin case between a significant deficiency and a material weakness	The staff responsible for recording revenue of fixed price contracts under the percentage-of-completion method did not have a clear understanding of when revenue and the related accounts receivable should be recognized. In addition, controls over cost-to-complete estimates were ineffective, resulting in changes without adequate documentation. No misstatements occurred, but the weaknesses could have resulted in an overstatement of revenue.	Significant Deficiency	Material Weakness

(continued on next page)

APPENDIX (continued)

Case	Case Summary	Management Classification of the ICFR Problem (manipulated between-participant)	
		Classification Set 1	**Classification Set 2**
Case 3—Harper (Revenue) At-margin case between a control deficiency and a significant deficiency	Poor controls existed over service revenue for the Latin American Division of the company. The accounting staff were not reviewing the contracts and underlying documents to support service revenue. This IC failure could have resulted in misstatement of service revenue that would not have been prevented or detected.	Control Deficiency	Significant Deficiency
Case 4—Konrath (Revenue and Inventory) At-margin case between a significant deficiency and a material weakness	The independent distributors did not have adequate controls over the recording of inventory resulting in a delayed posting of sales. This led to an underreporting of revenue amounting to 2.8 percent of net income in the first three quarters, which was detected before year-end. A second control weakness was detected where revenue was not recorded in accordance with shipping terms and when title had transferred. This resulted in underreporting of net income in the 3rd quarter. The error was detected and corrected prior to year-end.	Material Weakness	Significant Deficiency

(continued on next page)

APPENDIX (continued)

Case	**Case Summary**	**Management Classification of the ICFR Problem (manipulated between-participant)**	
		Classification Set 1	**Classification Set 2**
Case 5—Alexander (Fixed assets and Depreciation) At-margin case between a control deficiency and a significant deficiency	The company's fixed asset records are maintained on spreadsheet system that is not integrated with the financial accounting reporting system. There are inadequate controls to ensure that asset values, useful lives, and capitalization dates are recorded on a timely basis. No misstatement occurred but management acknowledges that misstatements in fixed assets and related depreciation expense could have resulted which would have not been prevented or detected.	Significant Deficiency	Control Deficiency
Case 6—Ballas (Inventory) At-margin case between a significant deficiency and a material weakness	Internal controls over the recording and maintenance of inventory (a significant part of the company's assets) were not operating effectively. The company relied on the external distributor's internal reporting system, but there was a lack of segregation of duties over the distributor's shipping and receiving and the inventory record-keeping functions. This resulted in an underreporting in inventory levels and an understatement of net income by 3.8 percent. The error was detected and corrected prior to issuing external financial reports.	Significant Deficiency	Material Weakness

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The Importance of Distinguishing Errors from Irregularities in Restatement Research: The Case of Restatements and CEO/CFO Turnover

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ABSTRACT: Research on restatements has grown significantly in recent years. Many of these studies test hypotheses about the causes and consequences of intentional managerial misreporting but rely on restatement data (such as the GAO database) that contains both irregularities (intentional misstatements) and errors (unintentional misstatements). We argue that researchers can significantly enhance the power of tests related to restatements by distinguishing between errors and irregularities, particularly in recent periods when the relative frequency of error-related restatements is increasing. Based on prior research, the reading of numerous restatement announcements, and the guidance that boards receive from lawyers, auditors, and the SEC on how to respond to suspicions of deliberate misreporting, we propose a straightforward procedure for classifying restatements as either errors or irregularities. We show that most of the restatements we classify as irregularities are followed by fraud-related class action lawsuits as compared to only one lawsuit in the group of restatements classified as errors. As further validation of our proxy, we report that the market reaction to the restatement announcement for our irregularities sample (−14 percent) is also significantly more negative than it is for our errors sample (−2 percent). Finally, we demonstrate the importance of distinguishing errors from irregularities by showing the impact it has on inferences about the relation between restatements and CEO/CFO turnover

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over time.

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Data Availability: *The data used in this study are available from public sources identified in the text. Our classification of the GAO sample is also available from the authors upon request.*

I. INTRODUCTION

Accounting research on the causes and consequences of financial restatements has grown significantly in recent years. Such work focuses primarily on those restatements arising from the misapplication of GAAP where the original financial statements were incorrect at the time issued. These misstatements can be classified as involving either *errors* (i.e., unintentional misapplications of GAAP) or *irregularities* (i.e., intentional misreporting) as defined by SAS No. 53 (AICPA 1988).[1] Studies often assume intentional misreporting (either explicitly or implicitly), but restatement samples still contain a mix of both errors and irregularities. We argue that the power of hypotheses tested on accounting restatements can be significantly improved either by limiting restatement samples to irregularities or by including a control variable distinguishing errors from irregularities.

Prior research supports the notion that investors, regulators, boards, and other stockholders differentiate between irregularities and errors. For example, Palmrose et al. (2004) report that the market reaction to restatement announcements related to fraud (i.e., deliberate misreporting) is −20 percent but is only −6 percent for non-fraud restatements. Samples constructed using only firms actually charged with financial fraud in the SEC's Accounting and Auditing Enforcement Releases (AAERs) would likely maintain a distinction between restatements caused by errors and those related to irregularities but would include only a subset of restatements caused by irregularities.[2]

Unfortunately, the larger General Accounting/Government Accountability Office (GAO) database, which is the most commonly used data source for recent restatement studies, does not contain a variable that differentiates between errors and irregularities.[3] Consequently, not all researchers separate intentional and unintentional misreporting, and there is considerable variation in the methods employed among papers that do make this distinction.[4] We believe that much of the failure to distinguish restatements in prior and concurrent research is due to the difficulty that researchers face in implementing a standardized approach for distinguishing among restatements in large samples.

In this study we propose a straightforward procedure for distinguishing errors from irregularities in a large sample of restatements. Ideally, all firms would explicitly disclose whether the restatement was caused by an irregularity or an error, but many do not. Since we cannot observe managerial intent and explicit disclosure by firms is sporadic, we construct our own procedure for classifying restatements as errors or irregularities. We read all restatement announcements and relevant subsequent filings and use three criteria to identify likely irregularities. Restatements meeting at least one of the following criteria are classified as irregularities, and all others are classified as errors.

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[1] Technically, an irregularity only rises to fraud if users rely on the financial statements to their detriment (Young 2002). However, the distinction between fraud and irregularities has become blurred over the years, and recent auditing guidelines (e.g., SAS No. 82, AICPA 1997) use the term "fraud" to refer to all intentional misstatements. We use the more inclusive term "irregularities" as there is still a subtle legal difference between irregularity and fraud.

[2] Limited resources, high burden of proof requirements, and granted leniency for self-remediation lead the SEC to pursue injunctive actions or recommend criminal charges in only a subset of cases of suspected fraud. Even among those firms listed in an AAER, only a subset are accused of fraud as Administrative Proceedings and Exchange Act Releases criticize firms' reporting without commenting on intent.

[3] The GAO has released two restatement databases that collectively cover restatements announced between January 1, 1997 and September 30, 2005.

[4] Examples of studies that assume (explicitly or implicitly) intentional misreporting but use a GAO sample that is a mix of both irregularities and errors include Arthaud-Day et al. (2006), Burns and Kedia (2006), Harris and Bromiley (2007), Kedia and Philippon (2008), and Lee et al. (2006).

We first classify any restatements using variants of the words "fraud" or "irregularity" in reference to the misstatement as irregularities. Given the litigation risk and reputational penalties associated with the disclosure of intentional misreporting, we believe these words are reliable when they are used. As our second criterion, we also classify restatements with related SEC or Department of Justice (DOJ) investigations as irregularities since these cases are more likely to involve managerial misbehavior. Last, we consider the presence or absence of other investigations into the accounting matter (e.g., the Audit Committee hires a forensic accounting firm) and classify restatements with related independent investigations as irregularities. As discussed more fully in Section II, our argument for classifying restatements involving independent investigations as irregularities is based on guidance that boards receive from auditors, lawyers, and the SEC on appropriate actions to take when confronted with suspicions of intentional misreporting.

For our primary analysis, we review all 8-K filings between 2002 and 2005 and identify restatements that correct previous misapplications of GAAP.[5] Using the criteria outlined above, we classify 24 percent of the restatements as irregularities and 76 percent as errors. This is consistent with recent reports from the SEC indicating that a substantial percentage of recent restatements are related to basic accounting mistakes or data errors.[6] Our classification identifies a larger sample of intentional misstatements than found in the AAERs or with the Palmrose et al. (2004) method. To illustrate, if we follow Palmrose et al.'s (2004) classification scheme for our 2002–2005 restatement sample, we identify 57 firms with a related AAER or that mention either "fraud" or "irregularity," which contrasts with the 105 irregularities we identify using our method.

We conduct several tests to validate our restatement classification to offer assurance that these additional observations are valid and that our classification scheme provides a meaningful partition that separates errors from irregularities. First, we compare the mean cumulative abnormal returns around the restatement announcements of the errors sample to the mean of the irregularities sample. The Palmrose et al. (2004) fraud evidence suggests that the market reaction should be much more negative for irregularities than for errors. One indication that our classification procedure is effective, therefore, would be a significant difference in the market reaction between the two groups. The mean (median) cumulative abnormal returns (CARs) for the error sample is −1.93 percent (−0.90 percent) compared to −13.64 percent (−19.4 percent) for the irregularities sample. The significantly more negative CARs for the irregularities sample provide validating support for the effectiveness of our procedure for separating irregularities from errors.

As a second validity test, we compare the frequency of securities class action lawsuits in the irregularities sample to the errors sample. A class action lawsuit alleging financial statement fraud would need to argue conscious misreporting, so such suits might exist for restatements related to irregularities but will likely not exist for those related to errors. If our partitioning procedure works well, then we should find that the class action lawsuits occur for firms in our irregularities sample but not in our errors sample. Consistent with this, we find that 84 of the 105 irregularities in our final sample had contemporaneous class action lawsuits, but only one of the 83 errors has a related lawsuit. Overall, both the CARs and the class action lawsuit analyses suggest that our procedure for partitioning errors and irregularities works well.

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[5] We obtain the restatement announcements from 8-K filings (the second GAO database had not yet been released at the time we began data collection), whereas the GAO searches news databases (e.g., Lexis-Nexis) to identify their restatements. However, our inclusion criteria are the same, and there is substantial overlap between the two samples.

[6] See, for example, remarks by Scott A. Taub, Acting Chief Accountant of the SEC, in his speech at the Financial Executives International Meeting on November 17, 2006 (http://www.sec.gov/news/speech/2006/spch111706sat.htm).

To demonstrate the importance of partitioning errors versus irregularities, we re-examine the relation between restatements and CEO/CFO turnover studied in earlier research. Although studies generally report that executive turnover rates around restatements are significantly different from turnover rates in a control sample of non-restaters, some question whether the observed turnover rates are too low to be consistent with effective board monitoring and strong corporate governance.[7] For example, in a recent study on restatements, Collins et al. (2005) conclude that "half the sample appear to have taken little or no action to penalize management." We contend that the lower than expected turnover rates observed in prior research are likely due to the mixing together of restatements due to both errors and irregularities.

Given that investors seem to react more strongly to restatements involving fraud, management's intention to deliberately misreport is likely to play a significant role in boards' decisions to terminate managers when these misstatements are identified. Furthermore, although the termination of a top executive may be partly to punish the manager for the loss in shareholder value caused by the restatement, the termination is also a highly visible means of restoring financial reporting credibility, something that Farber (2005) notes is an important priority for firms accused of fraud. Consequently, we predict CEO and CFO turnover rates will be higher for restatements due to irregularities than those due to errors.

As expected, once we partition the restatements, the turnover rates are much higher for irregularities than for errors. The percentage of restating firms experiencing turnover in the 13 months surrounding the restatements (six months before to six months after) is 49 percent (64 percent) for CEOs (CFOs) for our irregularities sample but only 8 percent (12 percent) for the errors. After excluding subsidiary-level restatements and expanding the turnover window to four years (two years before and two years after) to be more consistent with prior research, the (untabulated) turnover rates for irregularities restatements are 67 percent for CEOs and 85 percent for CFOs.[8] Furthermore, in 91 percent of irregularities, either the CEO or CFO leaves the firm. This evidence suggests that when the restatements are the result of intentional misbehavior (i.e., irregularities) rather than errors, boards consistently take swift action to dismiss managers.

To control for other factors that might give rise to CEO/CFO turnover, we estimate logistic regressions for both CEO and CFO turnover. We find that additional controls, including the magnitude of the restatement, the type of misstatement (i.e., revenue recognition versus other types), the annual or quarterly nature of the restatement, leverage, ROA, CEO ownership, and long-window (day −240 to day −8) CARs, do not diminish the significance of our severity measure (irregularity) in predicting executive turnover. Our multivariate tests show that both CEO and CFO turnover are significantly more likely if the restatement relates to an irregularity rather than to an error.

Finally, we demonstrate the importance of separating errors from irregularities by examining the changes in the causes and consequences of restatements over time. In the case of restatements and CEO or CFO turnover, one might argue that the increased political and regulatory pressure in the wake of Enron and other accounting scandals affected executive turnover rates around restatements. Senator Sarbanes' 2002 statement that managers who step outside of GAAP "ought to be punished, and punished very severely" may reflect an increased intolerance for managerial misreporting in the post-2002 period.[9] Despite its many critics, the Sarbanes-Oxley Act certainly aims to make widespread governance improvements. This raises the question of whether executive turnover around restatements may be more likely in the more recent regime than in prior periods.

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[7] For examples of turnover rates in studies where there is statistically significant difference in turnover rates for restatement firms see: Desai et al. (2006), Land (2006), Arthaud-Day et al. (2006), and Jayaraman et al. (2004).

[8] We show that most turnover (roughly 80 percent) linked to an irregularity occurs within a 13-month window surrounding the restatement announcement (six months before and six months after). Consequently, shorter windows (as opposed to windows as long as five years used in prior research) are likely more appropriate.

We compare executive turnover in our 2002–2005 sample to a sample of restatements in 1997–1998 that we construct from the 2003 GAO database (GAO 2003). Unexpectedly, frequency analysis (untabulated) suggests that the overall CEO and CFO turnover rates around restatements are lower in the later time period. However, the decrease in overall turnover is attributable to the decreasing proportion of irregularities among total restatements. We observe that the frequency of restatements caused by errors has gone up at a much faster rate than restatements caused by irregularities. Roughly 48 percent of the restatements in our 1997–1998 sample relate to irregularities as compared to only 24 percent in our 2002–2005 sample. This suggests that when assessing turnover rates across time it is important to also consider changes in the mix of restatements over the same periods. Once we eliminate restatements classified as errors, we find that the turnover rates for restatements due to irregularities have actually changed very little over time. Hence, our findings highlight the importance of distinguishing irregularities from errors to avoid making incorrect inferences for hypotheses involving managerial misconduct.

Although not a perfect measure of managerial intent (because management intent is impossible to actually observe), our procedure for identifying irregularities is fairly easy to implement and appears to be very effective at capturing important differences in restatements. For researchers, our findings suggest that partitioning on a proxy for irregularity can significantly enhance the power of tests in most studies related to restatements (particularly those that rely on the GAO database) and in certain cases may be necessary to avoid incorrect inferences. This is relevant not only for studies on turnover around restatements, but also for studies on other topics (e.g., insider trading, cost of capital, information content) that utilize a restatement setting.

The remainder of the paper is organized as follows. Section II discusses our restatement classification procedure. Section III describes our sample selection procedures, Section IV discusses the results of our turnover analysis, and Section V concludes.

II. RESTATEMENTS, THE GAO DATABASE, AND RELATED RESEARCH

A small sampling of research on accounting restatements includes such topics as: the market reaction to earnings restatements (Palmrose et al. 2004), restatements and the cost of capital (Hribar and Jenkins 2004), restatements and executive turnover (Desai et al. 2006; Jayaraman et al. 2004; Collins et al. 2005; Land 2006; Burks 2007), the information content of earnings after restatements (Wilson 2008), executive compensation and incentives to restate earnings (Burns and Kedia 2006; Efendi et al. 2007), and restatements and audit committee consequences (Srinivasan 2005). Although the subject matter varies widely, these studies generally have two things in common. First, these studies explore the causes and consequences of restatements with hypotheses based on the premise that restatements are due to aggressive accounting (i.e., intentional misreporting). For example, Kedia and Philippon (2008) study the economics of fraudulent reporting and refer to the misstated period for all the restatements in their sample as the "fraudulent reporting period."[10] Second, many of the recent restatement studies obtain restatement samples from the

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[9] Senator Paul S. Sarbanes, Senate Floor Statement on July 8, 2002 on the Public Company Accounting Reform and Investor Protection Act of 2002 (http://banking.senate.gov/pss/0708flor.htm).

[10] As further examples, the executive turnover studies hypothesize that executives are fired for aggressive accounting, and the compensation and restatement studies hypothesize that managers intentionally misstate earnings for compensation-related reasons.

GAO database.[11] Given the wide use of this database and the underlying assumption of deliberate misreporting made by most studies, further analysis of the types of restatements contained in the database is important for interpreting results in this line of research. If, for example, a large fraction of the restatements included in the GAO database are errors and not irregularities, then we can conclude that the power of the tests conducted could be improved by concentrating only on irregularities within the data set or by better controlling for this important restatement characteristic.

The GAO Database

On October 4, 2002, the GAO issued a report titled "Financial Statement Restatements: Trends, Market Impacts, Regulatory Responses, and Remaining Challenges" (GAO 2002). For this report, the GAO compiled a list of 919 restatements occurring between January 1, 1997, and June 30, 2002. This database of restatements was released to the public in early 2003, and a second database containing 1,390 restatements occurring between July 1, 2002 and September 30, 2005 was released in 2006 (GAO 2006). The combined database is comprised primarily of restatements arising from the misapplication of GAAP, which includes both errors (unintentional misstatements) and irregularities (intentional misstatements).[12]

The Chicago Bridge and Iron Works (CB&I) restatement is an example of a restatement arising from an irregularity that is contained in the 2006 GAO database. On October 31, 2005, CB&I announced that "the delay in releasing third quarter 2005 financial results was precipitated by a memo from a senior member of CB&I's accounting department alleging accounting improprieties."[13] The accusation of misbehavior prompted the board to launch an independent investigation that revealed deliberate accounting irregularities that would necessitate a restatement. This is representative of the type of misstatement that underlies the hypotheses being tested in a typical study that uses the GAO restatement database..

Restatements in the GAO database also arise from errors rather than irregularities. For example, the following is contained in a February 7, 2005 restatement by CECO Environmental Corporation that is included in the 2006 GAO database: "CECO's management detected a material misstatement in the systemic calculation in spreadsheets aggregating small project balances that the Company used to recognize revenue from such projects. Although revenue recognized under the percentage-of-completion calculation on individual large projects was accurate, due to this spreadsheet error, the aggregation of such totals for small and large jobs was incorrect."[14] CECO discovered this spreadsheet error during an internal controls evaluation. Although the error involves revenue recognition, there is no evidence that this restatement represents an overly aggressive accounting choice or deliberate misreporting. This is just one of many examples of restatements in the GAO database that is caused by an error rather than an irregularity.

The 2006 GAO database also includes a significant number of restatements related to lease accounting. Many retailers restated after the SEC issued a letter on February 7, 2005 clarifying their position on the treatment of certain lease features, and market reactions to these restatements were generally minimal. Applebee's International is an example of such a restatement: "On February 9, 2005...like many other companies in the restaurant, retail and other industries, it had determined that it would correct its accounting treatment for leases."[15] As the SEC's view on the proper treatment of a particular aspect of accounting for leases was not well understood prior to the SEC position letter and there is no evidence that Applebee's was deliberately attempting to mislead the market, this restatement is more like an error than an irregularity.

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[11] An August 1, 2007 Google Scholar search for "GAO database restatements" found 275 papers, which provides a rough estimate of the large number of published and working papers using the GAO database.

[12] Although SAS No. 53 specifically defines an irregularity as an *intentional* misstatement, the GAO uses "irregularity" to describe both the *intentional* and *unintentional* misstatements in their sample (GAO 2002, 76).

[13] http://www.sec.gov/Archives/edgar/data/1027884/000095012905010329/h29789exv99w1.htm.

[14] http://www.sec.gov/Archives/edgar/data/3197/000118143105007912/rrd67425.htm.

Many of the studies using the GAO database implicitly assume that the restatements are predominantly related to SAS No. 53 irregularities (i.e., intentional misstatements), but, as is evident from the CECO and Applebee's examples, there are in fact restatements arising from unintentional errors in the GAO database. Unfortunately, the GAO database does not include sufficient information to easily separate errors from irregularities. Consequently, not all researchers distinguish between these different types of misstatements, and the extent of the loss of power in hypothesis testing likely depends on the frequency of errors versus irregularities.

Distinguishing Errors from Irregularities

We argue that research on the causes and consequences of restatements can be significantly improved by distinguishing errors from irregularities for two reasons. The first is on theoretical grounds: as discussed above, the hypotheses being tested in the vast majority of restatement research assume that the financial misstatement was *intentional*.[16] For this line of research on misstatements, it is more appropriate to focus on irregularities rather than on errors, and it is possible that the inclusion of errors in a sample presumed to be entirely irregularities or "aggressive accounting" could affect the interpretation of the results. The second reason is that investors and regulators view irregularities as being much more severe, and controlling for the type (error versus irregularity) is likely to improve the power of tests in restatement settings.

The most compelling evidence for the difference in restatement severity is that provided by Palmrose et al. (2004) who report that the market reaction to restatements involving fraud (those with associated AAER or that explicitly disclose fraud/irregularity) is −20 percent as compared to only −6 percent for non-fraud cases. The authors argue that the stock price reaction is likely greater in cases of fraud for two reasons. First, the revelation of fraud is likely to increase the discount rate because it reduces the reliability of management disclosures. Second, the revelation of fraud leads to increased costs related to contemporaneous litigation and regulatory actions, additional future monitoring, and future regulatory scrutiny.

The Palmrose et al. (2004) evidence suggests that, depending on the research question and the extent to which a researcher's sample contains a mix of errors and irregularities, more powerful tests can be conducted by either controlling for the type of restatement (irregularity versus error) or by focusing exclusively on irregularities. As previously discussed, samples constructed using only firms actually charged with financial fraud in AAERs would maintain the distinction between restatements caused by errors and restatements related to irregularities but would likely include only a subset of the restatements due to irregularities.[17] However, although beginning with a larger alternate restatements database (such as the GAO database) allows for a more inclusive sample, it also requires a systematic way to distinguish misstatements caused by irregularities from those caused by errors.

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[15] http://www.sec.gov/Archives/edgar/data/853665/000085366505000063/restatement8k.txt.

[16] We note that not all restatement studies assume intent. For example, Srinivasan (2005) examines the consequences of audit committee failure, and in this case, even unintentional errors could be construed as a relevant failure of the audit committee. For other research, such as Aier et al. (2005) who investigate whether lack of financial expertise in the CFO position leads to more misstatements, an (unintentional) errors sample may be more appropriate.

[17] Although the AAER sample has the advantage of minimizing Type I classification errors (classifying a restatement as an irregularity when it is actually only an error), the SEC does not investigate or prosecute all irregularities. As discussed by Feroz et al. (1991) and GAO (2007), the SEC is most likely to pursue investigations or recommend charges for those cases where prosecution sends an important enforcement message or where the odds of success are high. Consequently, cases that are potentially more difficult to prosecute or have little impact will be excluded from a sample based solely on AAERs.

Since we cannot observe managerial intent, we construct our own procedure for classifying restatements as errors or irregularities. We first identify those firms that do use variants of the words "irregularity" or "fraud" in describing the misstatement as we have no reason to doubt these costly disclosures when they are observed. We read restatement announcements and relevant filings in the four years surrounding the restatement and identify 41 percent of our final irregularities sample from self-disclosure.

Although we have noted that the SEC does not pursue all suspected irregularities, we also argue (consistent with Feroz et al. [1991]) that restatements that do have related SEC investigations are most likely to be cases where intentional misreporting is suspected. Thus, we next search public filings for references to SEC or DOJ (i.e., Attorney General) investigations in the two years before and the two years after the initial restatement announcement.[18] Cumulatively, 81 percent of our final irregularities sample is identified by adding firms with SEC or DOJ investigations to the sample explicitly disclosing "irregularity" or "fraud."

As the last step in our classification procedure, we search restatement announcements and relevant filings in the four years around the restatement for references to (non-SEC) independent investigations.[19] Restatements with related independent investigations signal an irregularity or suspicions of an irregularity rather than just an error. Boards are instructed by lawyers, auditors, and the SEC to undertake such investigations when there is any suspicion that the misstatement is the result of intentional misreporting. The SEC encourages internal investigations and has demonstrated a willingness to reduce SEC sanctions and sometimes forgo an SEC enforcement action altogether if the firm undertakes an independent investigation, proactively contacts the SEC, and takes swift corrective action.[20] From a litigation risk perspective, legal experts encourage boards faced with a potential accounting irregularity to launch a special independent committee to undertake an investigation, to contact the SEC, and to issue a press release to convey to investors that previous financial statements cannot be relied upon (Young 2002). Perhaps most importantly, auditors must rely on management representations and cannot continue with unresolved suspicions of management fraud without exposing themselves to significant litigation risk. As part of their own risk management, auditors are likely to insist that the audit committee undertake an investigation whenever deliberate misreporting is suspected.

The board (or the audit committee specifically) could launch an independent investigation into accounting matters when there is concern that the misstatement reflects more than a simple error, but there are high costs associated with this decision that must be considered. As Bernile and Jarrell (2007) note in the backdating setting, the direct and indirect costs of an independent investigation can be substantial, and the announcement of such investigations are generally not well received by the market. Thus, although there are benefits to launching an independent investigation, there are also costs to tradeoff, and boards are more likely to initiate a costly investigation for cases where deliberate misreporting is suspected than for restatements due to errors.

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[18] There is often a four- to five-year lag between the restatement and any related AAER (Feroz et al. 1991). By searching for ongoing investigations rather than completed AAERs, we eliminate some of the time delay associated with the enforcement process, which allows us to use a more timely sample. We also retain firms under investigation where subsequent bankruptcy preempts any potential AAER.

[19] Investigations by the (independent) audit committee, by a special committee of outside directors, or by an outside forensic firm under the supervision of the audit committee are all considered independent. Internal investigations or reviews by management, "the company," or the current auditor are not deemed to satisfy this criterion.

[20] See AAER No. 1470 (http://www.sec.gov/litigation/investreport/34-44969.htm) for an example where the SEC cites the firm's prompt corrective actions as a reason for the reduced penalties.

To summarize, we classify restatements as likely being caused by irregularities if the firm explicitly uses variants of "fraud" or "irregularity" in discussion of the restatement, if there is a related SEC or DOJ investigation, or if there is an (non-SEC) independent investigation into the misstatement. A restatement not meeting any of these criteria is classified as involving only an error.

We note that the possibility of both Type I and Type II misclassifications exist. To the extent that irregularity firms do not disclose either an irregularity or an independent investigation and are not formally investigated by the SEC, restatements will be misclassified as errors (Type II). Additionally, any firms where the SEC or independent investigation later concludes that deliberate misconduct did not occur will be misclassified as restatements involving an irregularity (Type I). We believe such misclassifications are rare and would bias against finding differences between the two groups. We also believe that our partition represents a clear improvement in classification error over unclassified restatement samples while still offering a larger and timelier sample than a pure AAER sample.

Irregularities at the Parent-Level versus the Subsidiary-Level

In some restatements the GAAP violation is isolated to a foreign subsidiary or distinct business unit within the firm. When irregularities are isolated to the subsidiary, it is likely that the firm's overall financial reporting credibility will not be undermined to the same extent it would in the case of parent-level irregularities. For this reason we also distinguish between parent-level and subsidiary-level restatements.

III. SAMPLE SELECTION

Table 1 summarizes our restatement sample selection. We identify restatement firms by searching the 8-K filings on EDGAR from January 1, 2002 to June 15, 2006.[21] To be included in our sample, a firm must announce an earnings restatement and reference an accounting error, an irregularity, or some type of investigation into accounting matters. This procedure yields a total of 630 restatements.[22] Using the classification scheme described above, 460 (73 percent) of these restatements are classified as errors and 170 (27 percent) are classified as irregularities. We exclude firms in the financial services industry (SIC codes 6000–6999) as well as firms that do not have data available on Compustat. This leaves 361 restatements caused by errors and 132 restatements caused by irregularities.

Turnover and ownership data for later tests is hand-collected from filings and press releases. To reduce the cost of this data collection, we randomly select approximately 25 percent of the restatements arising from errors. We include all irregularities to maintain power in later tests that look only at irregularities. After reading restatement announcements, we also drop ten additional observations where the restatement had no income effect, which is consistent with prior research. Last, we eliminate 27 cases where there is ultimately no restatement or it is not yet completed. This leaves 83 errors and 105 irregularities for a total of 188 observations.

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[21] After August 14, 2004, the SEC now requires firms to file an 8-K with Item 4.02 (Non-Reliance of Previously Issued Financial Statements) whenever it is determined that previously issued financial statements should no longer be relied upon. This disclosure change likely enables us to identify more restatements after 2004 as it formalizes the requirement that firms file an 8-K for a restatement announcement and provides a separate category explicitly for restatements that facilitates our search procedures. We do not believe this biases our tests. The specific regular expressions used to identify restatements are available from the authors upon request.

[22] Consistent with prior work (e.g., Agrawal and Chadha 2005; Anderson and Yohn 2002; Burks 2007; Land 2006; Palmrose and Scholz 2004; Richardson et al. 2003; Salavei 2006), we exclude any "technical" restatements that do not imply a misstatement in the original filing (e.g., restatements for mergers, discontinued operations, change in principle).

TABLE 1
Sample Selection

	Errors	Irregularities	Total
Number of restatements identified from 8-K filings	460	170	630
Financial services	43	19	62
Firms not on Compustat	56	19	75
Total Available	361	132	493
Less firms not randomly selected	268		268
Observations	93	132	225
Less cases where restatement had no income impact	10	0	10
Less cases where investigation did not lead to restatement or is not complete	0	27	27
Final Sample	83	105	188
Weighting by population proportion	3.9	1.0	
Weighted Sample	324	105	429

Our sample was obtained as follows. We identified all 8-K disclosures where firms disclosed a restatement or intended restatement due to a misapplication of GAAP between January 1, 2002 and December 31, 2005. This includes 8-Ks filed specifically to announce a restatement, to announce a change in auditor, or to announce quarterly or annual financial statements. If a firm announced more than one restatement during this time period, then we selected the first restatement disclosed. This process yielded 630 restatements. Of these 460 are restatements arising from errors and 170 are from irregularities. We classify restatements as irregularities if the firm referred to the misstatement as an irregularity or announced an SEC/DOJ or other independent investigation into the accounting misstatement. Of these we exclude firms in the financial services industry (SIC codes 6000–6999) as well as firms that do not have data available on Compustat, which leaves us with 361 errors and 132 irregularities. To reduce the cost of data collection, we randomly select approximately 1 / 4 of the restatements caused by errors. We include all irregularities to maintain power in later tests for this group. From this we drop 10 observations from the error group where the restatement had no income impact. (These generally related to reclassifications or corrections to shares outstanding used in the computation of EPS.) Finally, we eliminate 27 irregularities where the investigation ultimately did not lead to a restatement or where the investigation has not yet been completed.

To adjust for the disproportional sampling (we select all irregularities but only a fraction of errors), we follow Manski and Lerman (1977) and weight by estimated population proportions. Error (irregularity) restatements are assigned a weight of 3.9 (1). The weight of 3.9 for the errors sample is computed by dividing the total number of restatements identified in the population (361) by the total number of observations sampled (93).

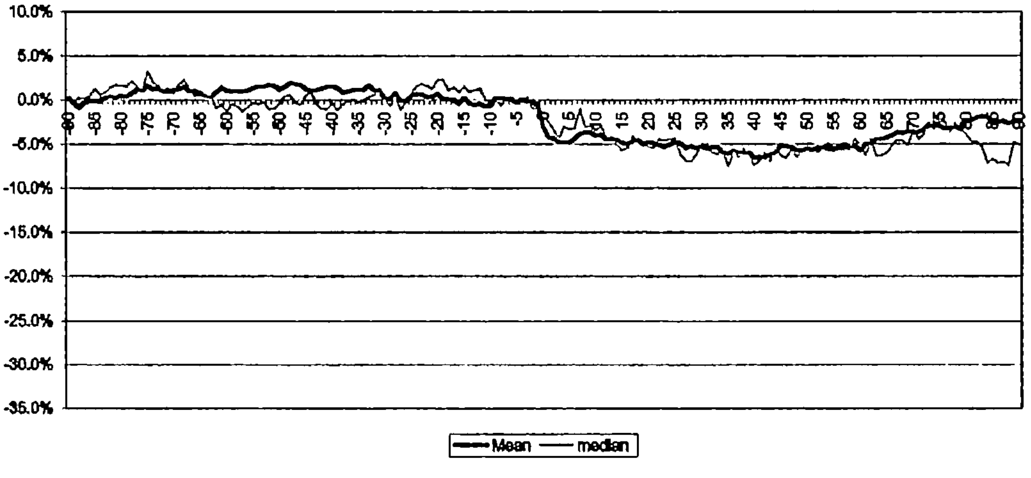
Validity of Irregularity Classification

We argue that irregularities are more severe than errors and that we can reasonably distinguish between irregularities and errors even when explicit disclosure is not provided. To validate that we are obtaining a meaningful partition, we analyze the stock returns between 90 days prior to 90 days following the restatement announcement. If irregularities are more severe and if we are correctly identifying irregularities, then the market reaction to the restatement announcement should be more negative for the irregularities sample than for the errors sample.

For comparison with prior research, Figure 1 first reports population-proportion adjusted mean and median CARs from *t*−90 to *t*+90 for the entire restatement sample. Consistent with prior research, there is a noticeable dip in returns in the days surrounding the restatement announcement with mean (median) CAR of −4.9 percent (−3.1 percent) from seven days before to seven days after the announcement. These returns are not as negative as reported in earlier research. For example, Palmrose et al. (2004) report mean (median) two-day CARs of −9.5 percent (−5.1 percent) for restatements announced between 1995 and 1999. This is potentially explained by a change in the mix of restatements over time, as suggested by Figure 5. Returns appear to be declining prior to the announcement, but we believe this decline is largely attributable to the difficulty in measuring the "information event." We use the date that the firm announces that it will restate earnings as the announcement date, but firms may disclose that they are investigating accounting issues or considering a restatement in an earlier press release. These earlier announcements likely lead to trading in anticipation of the restatement.

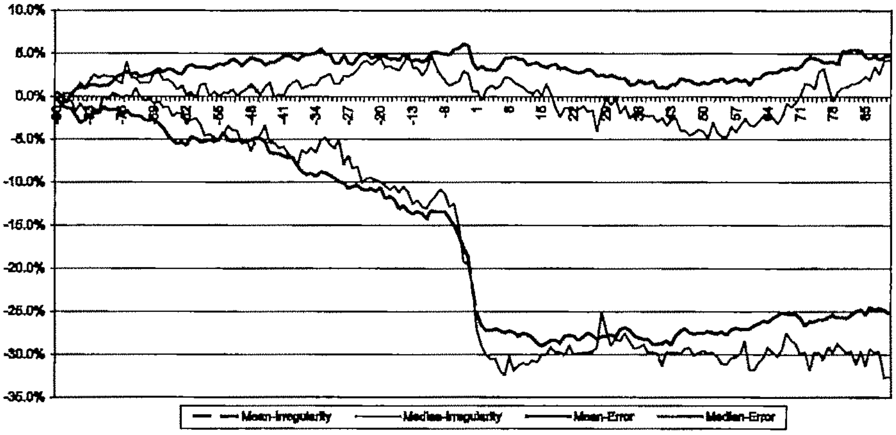
Figure 2 reports the mean and median CARs separately for errors and irregularities. The CARs for our errors sample do not drift far from zero over the entire 180-day period and exhibit very little reaction to the restatement announcement. The returns are actually positive leading up to the announcement but mean (median) CARs are −1.93 percent (−0.90 percent) in the 15-day window around the announcement. In contrast, the CARs for the irregularities group decline substantially. The mean (median) CARs are −13.64 percent (−19.14 percent) for the irregularity group around the same event window. In addition, the CARs drift −13 percent (−12 percent) between day −90 to day −8 for the irregularities. Again, this downward drift prior to the

FIGURE 1
Cumulative Abnormal Returns
180 Days Surrounding Restatement Announcement



This figure reports the mean and median cumulative abnormal returns for all restating firms beginning 90 trading days prior to the restatement announcement and ending 90 days after the restatements. Expected returns are CRSP value-weighted returns with dividends. To avoid survivor bias, we do not require observations to trade over the entire 180-day window. The number of observations ranges from 152 to 169.

FIGURE 2
Cumulative Abnormal Returns
180 Days Surrounding Restatement Announcement Grouped by Errors and Irregularities



This figure reports the mean and median cumulative abnormal returns for irregularities and errors. The number of observations ranges from 84 to 101 for irregularity and 68 to 69 for error restatements.

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announcement date suggests that a number of firms in our sample disclosed the possibility of a restatement before they announced definitively that they would restate earnings. Overall, this evidence is consistent with our classification of irregularities versus errors capturing the market's perception of the seriousness of the restatement.

To further validate that our procedure for identifying irregularities is effective, we compare the frequency of class action lawsuits claiming fraud for our error and irregularity samples. A class action lawsuit alleging financial statement fraud would need to argue conscious misreporting, so such suits might exist for restatements related to irregularities but will likely not exist for those related to errors. If our partitioning procedure works well, then we should find that the class action lawsuits occur for firms in our irregularities sample but not in our errors sample. Table 2, Panel A, reports a frequency count of errors and irregularities versus the filing of a class action lawsuit.[23] The first three columns of Panel A provide counts based on our classification criteria (announcement of an irregularity, regulatory investigation, or other independent investigation). Some firms have more than one of the required conditions (e.g., both an SEC investigation and an independent board investigation), so the three columns sum up to more than the "Total" column.

We find that 84 of the 105 irregularities in our final sample had contemporaneous class action lawsuits. Of the 21 cases without a class action suit, additional analysis (reported in Panel B) reveals that most of these restatements appear to be cases where there are limited benefits to filing a suit. For 17 of these 21 cases, one or more of the following conditions existed: the firm's stock was not being traded at the time of the announcement, the firm had a market value below $25

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[23] We obtained class action lawsuit information from Lexis-Nexis and the Stanford Class Action Clearinghouse (http://securities.stanford.edu).

TABLE 2

Class Action Lawsuits and Error/Irregularity Classifications

Panel A: Reason for Classification as Irregularity[a]

	Announced Irregularity	SEC/DOJ Investigation	Other Investigation	Total Irregularity	Total Error
Class Action Lawsuit	36	64	60	84	1
No Class Action Lawsuit	7	9	16	21	82
Total	43	73	76	105	83

Panel B: Classified as Irregularity but No Class Action Suit

	Count
Firms not being traded at time of announcement	3
Firms with market value below $25 million	8
Restatement was at the subsidiary-level	7
Firms with positive short-window returns	3
Total	21
Less multiple occurrences	(4)
Firms with at least one of the above	17
Misstatements coded as an irregularity for presence of SEC investigation, but no class action found	4
Total	21

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[a] Firms are classified into the irregularity sample if they disclose an irregularity, fraud, an SEC/DOJ investigation, or a board-initiated independent investigation. The irregularity columns do not sum up to "Total Irregularities" because some restatements have more than one type of related investigation (e.g., disclose "irregularity" and have an SEC investigation).

million, the restatement was at the subsidiary-level, or the firm had positive announcement returns. There were only four cases where the restatement is coded as an irregularity and there is no obvious reason to explain why a class action lawsuit was not filed. This again implies that we are properly identifying irregularities. It also suggests that our classification scheme is preferable to simply using the presence of a class action lawsuit to identify irregularities because using class action lawsuits will cause researchers to misclassify cases where fraud occurs but lawyers do not find it beneficial to sue (e.g., smaller firms or limited damages). We find only one case where a class action lawsuit was initiated but we did not code the restatement as being due to an irregularity. Overall, these findings suggest that classification appears to work quite well at distinguishing irregularities from errors.

IV. RESTATEMENTS AND CEO/CFO TURNOVER

To demonstrate the importance of distinguishing errors from irregularities, we re-examine the consequences that managers face for deliberate misreporting. Managers of restating firms likely face reduced compensation, decreased credibility, loss of employment, and even criminal charges depending on the severity of the GAAP violation. Although research on the effect of restatements on subsequent managerial compensation has recently emerged (e.g., Collins et al. 2005; Glass, Lewis, & Co., LLC 2005), we follow earlier research and focus on loss of employment as the most severe punishment implementable by the board of directors.

Surprisingly, some prior turnover studies find no evidence that restatements, even restatements linked to explicit fraud, significantly affect the odds of CEO turnover. For example, Beneish (1999) finds no difference in CEO turnover for firms that violate GAAP during 1987–1993 as compared to a control sample of compliant firms, and Agrawal et al. (1999) similarly find little evidence that firms suspected of fraud (including accounting fraud) between 1978 and 1992 have any higher executive turnover than non-fraud firms.

Other studies do find that accounting restatements increase the likelihood of managerial turnover, although the number of misstating firms that do *not* experience any turnover is still somewhat higher than some of these researchers expect. The percentage of firms experiencing executive turnover in prior samples varies depending on the number of executives and the time windows considered: Desai et al. (2006) find 60 percent of restating firms in 1997–1998 have turnover of their CEO, Chairman, or President within two years after restatement; Land (2006) estimates 45 percent of firms restating between 1996 and 1999 have CEO turnover in the year after the restatement; in the two years after the restatement, Arthaud-Day et al. (2006) observe CEO turnover in 43 percent and CFO turnover in 55 percent of their 1998–1999 sample of restating firms; and Jayaraman et al. (2004) find that 48 percent of their restating firms experience turnover of their CEO, Chairman, or President and 45 percent experience turnover of their CFO, Treasurer, or Controller in the four years after being listed in the 1999–2000 AAERs. Overall, past research suggests that a considerable portion of restating firms do not replace management in response to financial reporting failures.

A natural question given the existing evidence is whether the lower-than-expected turnover rates for restating firms is due to an agency problem, where boards fail to act expeditiously to remove senior management when they should, or because a large fraction of restatements are simple errors and do not warrant executive turnover.[24] Distinguishing between errors and irregularities should help to answer this question.

In addition to our error versus irregularity and parent- versus subsidiary-level classifications discussed in the previous section, we also control for other factors that prior research suggests are likely related to turnover. Prior restatement studies (e.g., Palmrose and Scholz 2004; Land 2006) argue that restatements of unaudited interim reports are viewed as less severe than restatements of audited annual reports, so we include a dummy for annual restatements. These studies also hypothesize that restatements of a larger dollar magnitude are more concerning to investors, so we include a scaled measure of the cumulative effect of the restatement (*MAGNITUDE*). We also control for the type of misstatement (*REVREC*) because prior work (e.g., Desai et al. 2006) has shown that revenue recognition restatements are associated with more negative CARs. To control for potential entrenchment effects such as those documented in Denis et al. (1997), we consider board and CEO ownership.[25] Many prior studies, including Gilson (1989), find that turnover is more likely for financially distressed firms, so we also control for distress with *LEVERAGE* (debt to total assets).[26] Finally, we control for past stock performance, measured as the CAR in the 240 trading days prior to the restatement announcement up to 8 days prior to the announcement. This controls for potential performance-related reasons for terminating the CEO/CFO, such as those documented by Warner et al. (1988) or Mian (2001). We also control for size by including indicator variables for size quintiles. To test our predictions for our severity measures while controlling for all the factors described above, we estimate the following model for CEO, CFO, and CEO or CFO turnover:

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[24] We note that there are costs to replacing an executive (e.g., severance pay, search costs) that make turnover sub-optimal in some circumstances, but these costs are likely to be dominated by reputational and auditor-reliance concerns in situations where intentional misreporting is suspected.

[25] We do not include CFO ownership in the CFO turnover model because the CFO is not always one of the five named executives for which beneficial ownership must be reported in the proxy statement.

$$\begin{aligned} TURNOVER = {} & \beta_0 + \beta_1 IRREGULARITY + \beta_2 SUBSIDIARY \\ & + \beta_3 IRREGULARITY{*}SUBSIDIARY + \beta_4 MAGNITUDE + \beta_5 REVREC \\ & + \beta_6 ANNUAL + \beta_7 CEO\_EQUITY + \beta_8 LEVERAGE + \beta_9 ROA \\ & + \beta_{10} CAR_{(t-240-t-8)} + \beta_{11}\text{Size Quintile } 1 + \beta_{12}\text{Size Quintile } 2 \\ & + \beta_{13}\text{Size Quintile } 4 + \beta_{14}\text{Size Quintile } 5 + \varepsilon \end{aligned} \tag{1}$$

where:

$TURNOVER$ = 1 if the CEO (or CFO) leaves the firm within the six months before or the six months after the restatement announcement, and 0 otherwise;
$IRREGULARITY$ = 1 if the restatement is classified as an irregularity, and 0 otherwise;
$MAGNITUDE$ = the cumulative amount of net income overstatement scaled by total assets in the year prior to the restatement announcement;[27]
$REVREC$ = 1 if any part of the GAAP violation relates to revenue recognition, and 0 otherwise;
$SUBSIDIARY$ = 1 if the GAAP violation is isolated in a subsidiary, and 0 otherwise;
$ANNUAL$ = 1 if the firm restated a 10-K, and 0 if the firm restated only 10-Qs;
CEO_EQUITY = the percentage of equity ownership of the CEO in the year prior to the restatement;
$LEVERAGE$ = Debt (#9 + #34) / Assets (#6);
ROA = operating income after depreciation scaled by total assets (Compustat #178/#6);
$CAR_{(t-240-t-8)}$ = the firm's cumulative abnormal returns from 240 trading days prior to the restatement announcement to 8 trading days prior, where expected returns are the CRSP value-weighted returns inclusive of dividends; and
Size Quintile i = indicator variable for size quintiles based on total assets.

The Turnover Window

In contrast to many prior restatement studies, we consider turnover both before and after the announcement date for two reasons. First, if management turnover occurs prior to the announcement, it may be the case that new management discovered GAAP violations perpetuated by prior management. In such a case, we would not expect current management to be dismissed for the acts of previous management. If these pre-announcement management changes are not considered, then researchers will incorrectly conclude that the boards failed to take swift action to terminate at-fault managers. A second reason to include the pre-restatement announcement period is that an initial investigation into irregularities can occur many months before a definitive restatement is publicly announced. CEOs and CFOs may be terminated during an investigation but before the restatement is announced. These measurement problems suggest that expanding the turnover window to include a timeframe before the restatement announcement is appropriate.

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[26] We used the Altman (1968) Z-Score as an alternative measure of financial distress with comparable results. Tabulated results use leverage, rather than Z-score, as it allows us to retain more observations.

[27] Our *MAGNITUDE* variable is consistent with the measure used in Palmrose et al. (2004) and Srinivasan (2005).

In our primary analysis, we use a 13-month window around (six months before and six months after) the restatement announcement.[28] However, given the lack of consensus in past research on the appropriate measurement window for executive turnover around restatements, we review the timing of turnover to assess the most appropriate window. We plot the cumulative turnover rates for restatements caused by errors and irregularities beginning six months before to two years after the restatement announcement. Figure 3 plots the CEO cumulative turnover rates beginning six months prior to the restatement announcement for our entire 2002–2005 sample. The plot shows that most of the turnover for both *IRREGULARITIES* and *ERRORS* occur very close to the announcement—between six months before and six months after the restatement announcement. In the *IRREGULARITY* sample, (untabulated) a total of 50 firms (89 percent of all firms with CEO turnover) announce the departure of their CEO over this time period. Very little turnover occurs after six months following the restatement announcement. The turnover rate is much lower for *ERROR* restatements (a total of 9), but it is still concentrated around the five months before and five months after the announcement where seven CEO departures (77 percent) are announced.

Figure 4 plots cumulative CFO turnover for our entire 2002–2005 sample. As in the case of CEO turnover, most of the CFO turnover occurs close to the restatement announcement. Roughly 92 percent of the CFO turnover (63 out of 79) occurs between six months prior to and six months following the restatement announcement of an *IRREGULARITY*. CFO turnover is less concentrated for the *ERROR* cases where 10 out of 18 restatements (55 percent) occur between six months prior to and six months following the restatement.

Overall, this analysis suggests that most turnover occurs close to the restatement announcement and that using windows that cover six months before and six months after the restatement will safely capture most of the turnover related to restatements. More importantly, the fact that most of the turnover occurs close to the restatement suggests that the turnover is likely related to the restatement.[29] Using longer windows will tend to introduce more noise into the analysis (i.e., more cases of turnover unrelated to the restatement).

Descriptive Statistics

Descriptive statistics are reported in Table 3. The table contains statistics for our 2002–2005 *ERROR* and *IRREGULARITY* samples. To mitigate the impact of outliers, all variables are winsorized at the bottom and top 1 percent. All variables are measured in the year prior to the restatement announcement.

The *IRREGULARITY* firms appear to be poor performers relative to the *ERROR* firms: *ROA* is significantly lower for the *IRREGULARITY* group (mean = −1.8 percent, median = 3.0 percent) compared to the *ERROR* group (mean = 3.3 percent and median = 5.6 percent) as is net income/assets. The *MAGNITUDE* of the restatement is also significantly higher for the *IRREGULARITY* group (mean = 11.9 percent, median = 1.50 percent) than for the *ERROR* group (mean = 4.0

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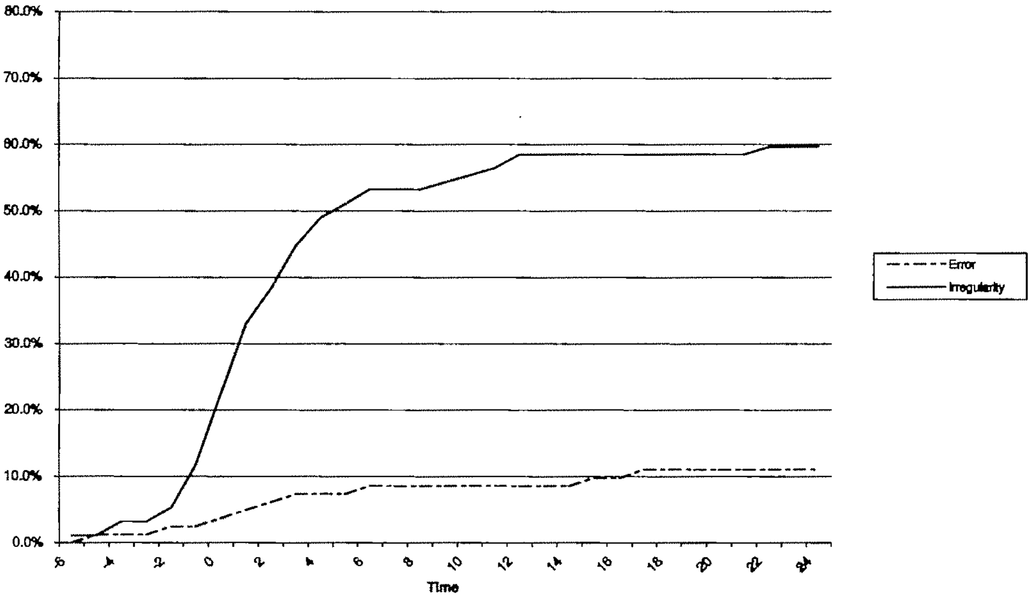
[28] Identifying exact announcement dates related to restatements is challenging. We use the date that a definitive intention to restate is first announced (not the date that an initial investigation or a potential restatement is announced).

[29] We find that the vast majority of the turnovers in this window would be classified as forced using the methodology employed by Parrino (1997).

FIGURE 3
Percentage of Firms with CEO Turnover over Time Relative to Restatement Announcement Date

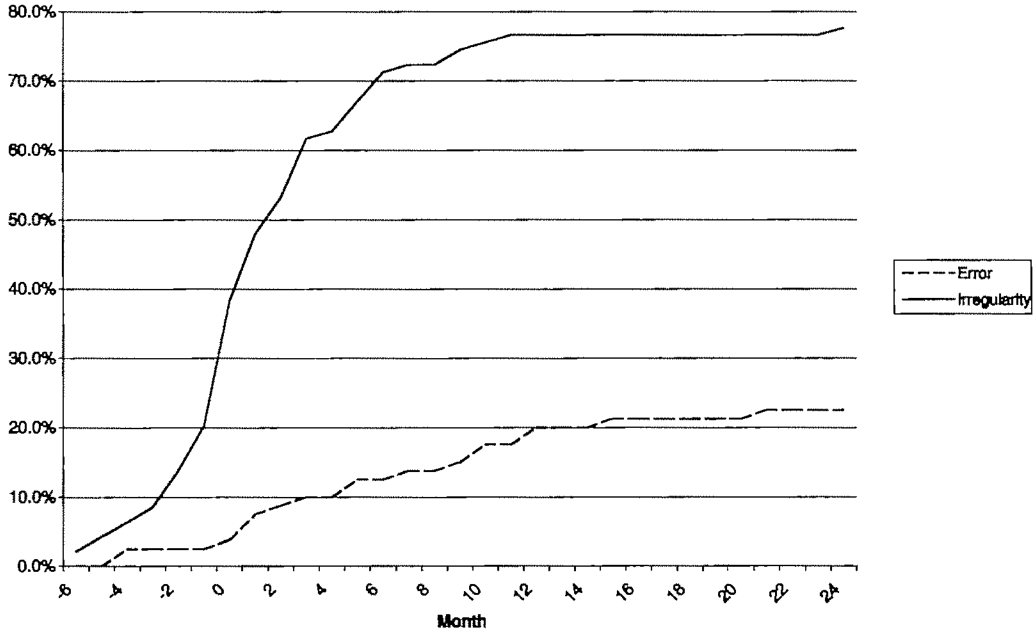


This figure reports the cumulative CEO turnover percentage from month −6 to month +24, relative to the date of the restatement announcement, which is the first date the firm announces that it will restate earnings (it is possible that the firm previously announced an investigation or potential restatement). A restatement is classified as an irregularity if the announcement explicitly refers to an irregularity as the reason for the restatement or discloses either an SEC investigation or an independent board investigation, and 0 otherwise. Sample details are reported in Table 1. If a firm has turnover more than once during the 30-month window, then we select the turnover that occurred closest to the restatement announcement and do not count other turnovers that occur, which understates the overall turnover rates.

percent and median = 0.7 percent). This is expected as managers are unlikely to intentionally depart from GAAP to affect small changes in earnings and are less likely to make income-decreasing misstatements. In contrast, errors can appear in any part of the distribution outside the materiality threshold.

Irregularities are also significantly more likely to involve revenue recognition issues. Roughly 59 percent of the misstatements in the *IRREGULARITY* sample involve revenue recognition issues compared to only 16 percent for the *ERROR* sample. The percentage for the *IRREGULARITY* group is similar to that reported in Dechow et al. (2007), who report that 54 percent of AAERs involve revenue manipulation, and is consistent with revenue being easier to manipulate than other income statement items. Since errors can occur anywhere in the accounting system, it is not surprising that revenue recognition makes up a smaller percentage of total misstatements in the *ERROR* sample. Total assets are weakly significantly higher for the *IRREGULARITY* group at

FIGURE 4
Percentage of Firms with CFO Turnover over Time Relative to Restatement Announcement Date



This figure replicates Figure 3 for CFO turnover.

the mean but are insignificantly different at the median. The two groups appear similar on all other reported dimensions.

Univariate Analysis

Table 4 reports frequency information on CEO and CFO turnover rates across various groups for our 2002–2005 sample. Beginning with the 324 restatements classified as *ERRORS* (actual count is 83 but count is weighted by 3.9 for comparability to the intentional sample), we find that the turnover rate is higher for CFOs than for CEOs, possibly because even an error is viewed as a control failure. More specifically, we observe that 8.4 percent (12.0 percent) of *ERROR* firms have a turnover of the CEO (CFO) between six months before and six months after the restatement announcement and in 15.7 percent of these firms at least one of the CEO or CFO turns over. These turnover rates for the *ERROR* cases are comparable to rates reported for non-restatement samples, but the turnover rates for the *IRREGULARITY* cases are much higher. In the *IRREGULARITY* cases, 48.6 percent (63.8 percent) of the firms experience turnover of the CEO (CFO) with 73.3 percent losing either a CEO or CFO.[30] These significantly higher rates imply that a CEO (CFO)

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[30] It is difficult to compare the turnover rates that we document to prior restatement research because past research investigates different time periods, different groupings of officers, and different window lengths.

TABLE 3
Descriptive Statistics

	Error				Irregularity			
	n	**Mean**	**Median**	**Std.**	**n**	**Mean**	**Median**	**Std.**
Sales	80	2725	533	6356	94	3984	497	8228
Sales Growth	80	14.7%	8.3%	42.4%	94	24.0%	4.7%	102.1%
Total Assets	82	2788	405	6857	104	4775*	569	9683
Market Value	73	2247	387	5544	93	3212	436	7250
ROA	82	3.3%	5.6%	13.6%	104	−1.8%**	3.0%###	17.6%
Income/Assets	80	−2.3%	2.3%	16.5%	94	−14.2%***	−3.6%###	34.7%
LEVERAGE	82	0.25	0.17	0.27	103	0.29	0.25	0.24
MAGNITUDE	81	4%	0.7%	16.6%	93	11.9%*	1.5%##	38.6%
REVREC	83	0.16	0	NA	105	0.59***	1###	NA
CEO_EQUITY	83	9%	3%	15%	104	7%	2%	15%

***, **, * Indicate t-tests of differences across groups significant at $p < 0.01$, 0.05, and 0.10, respectively.

###, ##, # Indicate Wilcoxon signed-rank tests across groups significant at $p < 0.01$, 0.05, and 0.10, respectively.

Details of the sample selection procedure for restatement firms are provided in Table 1. Variables listed above are those reported in the year prior to the restatement (not restated). Sales is Compustat #12. Sales growth is the change in sales from $t-2$ to $t-1$ scaled by sales in $t-2$. Total assets is Compustat #6. Market Value is Compustat #25*#199. *ROA* is operating income after depreciation scaled by assets (Compustat #178/#6). Income/Assets is Compustat #172/#6. *LEVERAGE* is Debt/Assets (Compustat (#9 + #34) / #6). *REVREC* is 1 if the misstatement is partly related to revenue recognition, and 0 otherwise. *MAGNITUDE* is the amount of net income overstatement scaled by total assets in the year prior to the restatement announcement. *CEO_EQUITY* is the fraction of CEO ownership in the year of the restatement. In cases where we are unable to obtain ownership information in the year of the restatement, we use the prior year.

The amounts reported above do not always capture the restated amounts. Whether the Compustat values are the initially reported amounts or the restated amounts depends on the timing of the restatements. If, for example, a firm with fiscal year-end of December 31, 2005 files a 10-K in March 2006 but later amends that filing prior to Compustat's next "cut" of the database, say November 2006, then Compustat uses the November 2006 data and ignores the original filing (in March 2006). In these cases, the data reported in this table are the restated figures. If, on the other hand, a company amends a prior year after Compustat's next "cut" of the data, then the restated information will appear in Compustat's special restatement variables. In these cases, our descriptive statistics will not include the restated amounts.

leaves more frequently around misstatements due to *IRREGULARITIES* than around those dealing with *ERRORS*, which is consistent with our expectation that boards are more likely to terminate CEOs and CFOs when the GAAP violation that necessitated the restatement is perceived as intentional.

We next compare the turnover rates of the subsidiary-level restatements to the parent-level restatements. As expected, in the *IRREGULARITY* sample the turnover rates for subsidiary-level restatements are lower than for parent-level restatements. CEO (CFO) turnover rates for subsidiary-level restatements are 29.2 percent (41.7 percent), which is significantly lower than the 54.3 percent (70.4 percent) for parent-level restatements. This is consistent with the firm attributing the *IRREGULARITY* misstatement to managers at the subsidiary. Supporting this explanation, in virtually every case where we classify the restatement as an *IRREGULARITY* at the subsidiary-level, we find that the firm announced that the subsidiary-level managers were terminated. In untabulated results, we find that turnover rates for parent-level restatements are extremely high when a four-year window (two years before to two years after) is considered. Using this alternate

TABLE 4
Turnover Frequency

	Error Turnover %				Irregularity Turnover %				Total Turnover %			
	n	CEO	CFO	CEO or CFO	n	CEO	CFO	CEO or CFO	n	CEO	CFO	CEO or CFO
Total	324	8.4	12.0	15.7	105	48.6^{a}	63.8^{a}	73.3^{a}	429	18.3	24.7	29.8
Subsidiary- vs. Parent-Level												
Subsidiary-Level	43	9.1	9.1	9.1	24	29.2	41.7	50.0	67	16.3	20.8	23.8
Parent-Level	281	8.3	12.5	16.7	81	54.3^{b}	70.4^{b}	80.2^{c}	362	18.6	25.5	30.9
Annual vs. Quarterly												
Annual	281	8.3	11.1	15.3	91	49.5	67.0^{d}	74.7	372	18.4	24.8	29.8
Quarterly	43	9.1	18.2	18.2	14	42.9	42.9	64.3	57	17.4	24.3	29.5

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[a] Chi-square contingency test statistic, comparing turnover rates in the error and irregularity samples significant at $p < 0.01$.

[b] Chi-square contingency test statistic, comparing turnover rates for parent-level versus subsidiary-level restatements in the irregularity sample, significant at $p < 0.05$.

[c] Chi-square contingency test statistic, comparing turnover rates for parent-level versus subsidiary-level restatements in the irregularity sample, significant at $p < 0.01$.

[d] Chi-square contingency test statistic, comparing turnover rates for annual versus quarterly restatements in the irregularity sample, significant at $p < 0.10$.

This table summarizes the turnover rates for restatements caused by errors and irregularities, further partitioned on subsidiary-level/parent-level and annual/quarterly. Sample selection and determination of error versus irregularity classification are described in Table 1. A subsidiary-level restatement is defined as a restatement that occurred in a subsidiary or foreign branch. All other restatements are considered to be parent-level restatements. Annual restatements are those that required restatement of a 10-K filing. Quarterly filings are those that only affected 10-Q filings. Turnover is considered to have occurred if a CEO or CFO left the firm in the six months before to six months after the restatement or investigation announcement, whichever is first. The frequency counts for the error restatements are weighted by population proportion for comparison to the irregularity restatement sample. As discussed in Table 1, we selected a random sample consisting of approximately 1 / 4 of the error cases that we identified but analyze all irregularity cases. To approximate the total turnover we would expect to observe in the population of all restatements, we assign 3.9 times the weight to the error cases.

window for parent-level *IRREGULARITY* restatements, we find that the CEO (CFO) turns over in 66.7 percent (85.2 percent) of the firms; in 91.4 percent of the cases either the CEO or CFO leaves the firm.

Finally, we compare turnover related to annual restatements to those of quarterly restatements. Given that quarterly financial statements have not undergone the audit process, we expect quarterly restatements to be viewed as being less severe than annual restatements. We consider a restatement to be annual when a 10-K is restated and quarterly when only 10-Qs are restated. The frequency of quarterly restatements in our sample is much lower than annual restatements. There are 372 annual restatements (281 *ERRORS* and 91 *IRREGULARITIES*) and only 57 quarterly restatements (43 *ERRORS* and 14 *IRREGULARITIES*). The incidence of CFO turnover within the *IRREGULARITY* group is (weakly) significantly higher for annual restatements than for quarterly and is insignificantly higher for CEO or combined CEO/CFO turnover.

In summary, we observe that turnover appears to be strongly related to our proxy for the underlying intent behind the GAAP violation. *IRREGULARITIES* appear to lead to greater turnover of both CEOs and CFOs, but this effect is reduced when the problem can be isolated to a subsidiary. In the next section, we provide logistic regression analysis to further support these observations.

Multivariate Analysis

Our observations from Table 4 support our predictions that perceived intentional GAAP violations give rise to higher CEO and CFO turnover rates and that the turnover rates for *IRREGULARITY* restatements will be lower when the irregularity causing the restatement occurs at the subsidiary-level. However, other factors that cause turnover are also likely to be correlated with intentional GAAP violations. To control for these factors, we estimate logistic regressions for CEO, CFO, and CEO or CFO turnover.[31]

Table 5 reports results for our 2002–2005 turnover regressions. Consistent with our prediction, the coefficient on *IRREGULARITY* is positive and significant ($p < 0.01$). (All p-values are reported as one-tailed when the sign is predicted.) Given a parent-level restatement, a CEO is more likely to turn over if the restatement was an intentional violation rather than an error. Evaluating all other variables at the mean, the predicted probability of CEO turnover is 55.7 percent for the irregularity sample and 7.9 percent for the error sample when *SUBSIDIARY* = 0. The predicted probability of CEO turnover falls to 22.4 percent for the irregularity sample and 6.7 percent for the error sample when *SUBSIDIARY* = 1. Hence, a firm with a restatement classified as an irregularity is roughly seven times more likely to have CEO turnover than a firm with an error if the restatement occurs at the parent-level but is only about three times higher than errors when the restatement occurs at the subsidiary-level. The coefficient on *IRREGULARITY*SUBSIDIARY* is insignificant, but untabulated tests using the method outlined in Ai and Norton (2003) reveal that the interaction effect is significantly negative for some observations. A negative interaction effect is consistent with the relative impact of an irregularity on the likelihood of turnover being reduced when the problem can be isolated to a subsidiary. Of the control variables, only *CEO_EQUITY* is significant in the expected direction ($p < 0.05$). The negative coefficient on *CEO_EQUITY* is consistent with the CEOs using their ownership in the firm to make it more difficult for boards to fire them.

Results for CFO turnover are very similar to our CEO turnover results. The coefficient on *IRREGULARITY* is 3.25 (significant at $p < 0.01$), suggesting that, conditional on a parent-level restatement, a CFO is more likely to turn over if the restatement is an *IRREGULARITY* rather than an *ERROR*. Evaluating all other variables at the mean, the predicted probability of CFO turnover is 74.5 percent for the irregularity sample and 10.2 percent for the error sample when *SUBSIDIARY* = 0. These predicted probabilities fall to 33.0 percent and 8.8 percent for irregularities and errors, respectively, when *SUBSIDIARY* = 1. Hence, a firm with a restatement classified as an irregularity is roughly seven (four) times more likely to have CFO turnover than a firm with an error when the restatement occurs at the parent- (subsidiary-) level. The negative coefficient on $CAR_{(t-240-t-8)}$ is significant ($p < 0.05$), which is consistent with CFOs of poorer performing firms being more likely to turn over. The coefficient on *LEVERAGE* is also (weakly) significantly positive in the CFO regression ($p < 0.10$), suggesting that turnover is more likely for more distressed firms. Finally, the size controls suggest that the probability of CFO turnover declines with firm size.

When CEO and CFO turnover are combined, results are very similar. Evaluating all other variables at the mean, the predicted probability of CEO or CFO turnover is 82.1 percent for the

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[31] Given the strong relation between bankruptcy and CEO turnover described in past research (e.g., Beneish 1999) we also test the sensitivity of our results to bankruptcy filings. We identify all firms (21 irregularity and 2 error) that file for bankruptcy at any point after the restatement announcement. Results are similar after excluding bankruptcies.

TABLE 5

Logistic Regression
Turnover

$$TURNOVER = \beta_0 + \beta_1 IRREGULARITY + \beta_2 SUBSIDIARY + \beta_3 IRREGULARITY{*}SUBSIDIARY + \beta_4 MAGNITUDE + \beta_5 REVREC + \beta_6 ANNUAL + \beta_7 CEO\_EQUITY + \beta_8 LEVERAGE + \beta_9 ROA + \beta_{10} CAR_{(t-240-t-8)} + \beta_{11}\text{Size Quintile 1} + \beta_{12}\text{Size Quintile 2} + \beta_{13}\text{Size Quintile 4} + \beta_{14}\text{Size Quintile 5} + \varepsilon$$

	Pred. Sign	CFO Turnover	CFO Turnover	CEO or CFO Turnover
Intercept		−1.94**	−1.81*	−1.09
		(5.47)	(5.03)	(2.08)
IRREGULARITY	+	2.68***	3.25***	3.23***
		(21.59)	(23.17)	(26.64)
SUBSIDIARY	?	−0.18	−0.16	−0.62
		(0.04)	(0.03)	(0.44)
*IRREGULARITY*SUBSIDIARY*	−	−1.29	−1.62	−1.29
		(1.13)	(1.62)	(1.12)
MAGNITUDE	+	0.71	−0.50	−0.45
		(0.50)	(0.28)	(0.20)
REVREC	+	−0.30	0.32	0.21
		(0.33)	(0.36)	(0.16)
ANNUAL	+	0.08	0.40	0.09
		(0.02)	(0.31)	(0.02)
CEO_EQUITY	−	−0.05**	−0.01	−0.03*
		(3.86)	(0.66)	(1.95)
LEVERAGE	+	−0.92	1.36*	1.01
		(0.82)	(2.44)	(1.45)
ROA	−	0.57	−1.21	0.36
		(0.10)	(0.40)	(0.04)
$CAR_{(t-240-t-8)}$	−	−0.10	−0.86**	−0.87**
		(0.04)	(3.02)	(3.24)
Size Quintile 1	?	0.66	−0.23	−0.18
		(0.38)	(0.04)	(0.03)
Size Quintile 2	?	1.02*	−0.33	−0.15
		(1.68)	(0.20)	(0.05)
Size Quintile 4	?	0.10	−1.39**	−1.14**
		(0.03)	(4.15)	(3.39)

(continued on next page)

TABLE 5 (continued)

	Pred. Sign	CFO Turnover	CFO Turnover	CEO or CFO Turnover
Size Quintile 5	?	−0.32	−2.40***	−1.69***
		(0.25)	(10.01)	(6.64)
Pseudo-R^2 (%)		33.26	50.37	49.04
Log Likelihood		41.98	74.78	75.58
n		172	172	172

***, **, * Represent p-values < 0.01, 0.05, and 0.10, respectively (p-values are reported as one-tailed when the sign is predicted).

Logistic regressions of variations of model (1) are reported above. Chi-square statistics are in parentheses. Pseudo-R^2 is the Nagelkerke (1991) Pseudo-R^2. Sample selection information is detailed in Table 1. We lose 16 observations due to missing data. To approximate the sample proportions we would expect to observe in the population of all restatements, we estimate the logistic regression assigning approximately 3.9 times the weight to the error cases and then normalizing the sample size. The dependent variable, *TURNOVER*, is 1 if the executive left the firm in the six months before or six months after the restatement or investigation announcement, whichever is first, and 0 otherwise. *IRREGULARITY* is 1 if the restatement announcement explicitly refers to an irregularity as the reason for the restatement or discloses either an SEC investigation or an independent board investigation, and 0 otherwise. *SUBSIDIARY* is 1 if the restatement occurred in a subsidiary, and 0 otherwise. *MAGNITUDE* is the amount of net income overstatement scaled by total assets in the year prior to the restatement announcement. *REVREC* is 1 if the misstatement is partly related to revenue recognition, and 0 otherwise. *ANNUAL* is 1 if the firm restated a 10-K, and 0 if the firm restated only 10-Qs. *CEO_EQUITY* is the fraction of CEO ownership in the year of the restatement. *LEVERAGE* is Debt (#9 + #34) / Assets (#6). *ROA* is operating income before interest and taxes scaled by assets (Compustat #178/#6). $CAR_{(t-240-t-8)}$ is the firm's cumulative abnormal returns from 240 trading days prior to the restatement announcement to 8 trading days prior, and expected returns are the CRSP value-weighted returns inclusive of dividends.

irregularity sample and 15.3 percent for the error sample when the misstatement occurs at the parent-level ($SUBSIDIARY = 0$). The turnover rates are 40.4 percent and 8.9 percent for irregularities and errors, respectively, when the misstatement occurs at the parent-level. Hence, a firm with a restatement classified as an irregularity is roughly 5.3 (4.6) times more likely to have CEO or CFO turnover than a firm with an error when the misstatement occurs at the parent- (subsidiary-) level. In all three models we fail to find significant evidence that *MAGNITUDE* or *REVREC* have any effect on executive turnover after we control for *IRREGULARITY*. Consistent with prior literature, we continue to find some evidence that prior performance and managerial ownership are also related to executive turnover.

Given the CAR distributions reported in Figures 1 and 2, one question is whether our partition on irregularity/error is any better than simply using restatement announcement returns. In other words, *IRREGULARITY* could simply be capturing the market reaction to restatements, which is an overall measure of severity. In Table 6 we evaluate the effectiveness of this alternative proxy by re-estimating our model substituting announcement CARs ($CAR_{(t-7-t+7)}$) for *IRREGULARITY*. For all turnovers (CEO, CFO, CEO/CFO), the main effect, $CAR_{(t-7-t+7)}$, is similar in sign and significance to *IRREGULARITY*. However, the explanatory power of the model (measured by Pseudo-R^2 or log likelihood) is much lower. For example, the log likelihoods using $CAR_{(t-7-t+7)}$ as a proxy for severity are 21.21, 43.68, and 40.34 compared with log likelihoods of 41.98, 74.78, and 75.58 when using *IRREGULARITY* as a proxy for severity, for the CEO, CFO, and CEO/CFO

TABLE 6
Turnover Logistic Regression
Short-Window CARs as Severity

$$TURNOVER = \beta_0 + \beta_1 CAR_{(t-7-t+7)} + \beta_2 SUBSIDIARY + \beta_3 CAR_{(t-7-t+7)} * SUBSIDIARY \\ + \beta_4 MAGNITUDE + \beta_5 REVREC + \beta_6 ANNUAL + \beta_7 CEO\_EQUITY + \beta_8 LEVERAGE \\ + \beta_9 ROA + \beta_{10} CAR_{(t-240-t-8)} + \beta_{11}\text{Size Quintile } 1 + \beta_{12}\text{Size Quintile } 2 \\ + \beta_{13}\text{Size Quintile } 4 + \beta_{14}\text{Size Quintile } 5 + \varepsilon$$

	Pred. Sign	CEO Turnover	CFO Turnover	CEO or CFO Turnover
Intercept		−1.56*	−1.50*	−0.99
		(3.90)	(3.65)	(1.82)
$CAR_{(t-7-t+7)}$	−	−3.07**	−3.50**	−2.51*
		(2.97)	(3.77)	(2.19)
SUBSIDIARY	?	−0.95	−1.44*	−1.73**
		(1.35)	(2.60)	(4.02)
$CAR_{(t-7-t+7)}$ **SUBSIDIARY*	−	−0.23	−2.65	−4.10
		(0.01)	(0.43)	(1.00)
MAGNITUDE	+	0.85	0.12	0.22
		(0.25)	(0.01)	(0.02)
REVREC	+	0.26	0.83**	0.86**
		(0.28)	(2.93)	(3.49)
ANNUAL	+	0.29	0.40	0.22
		(0.20)	(0.33)	(0.13)
CEO_EQUITY	−	−0.07**	−0.02	−0.02
		(3.37)	(0.47)	(1.16)
LEVERAGE	+	0.10	1.71**	1.50**
		(0.01)	(3.55)	(3.17)
ROA	−	−0.47	−1.49	−0.27
		(0.07)	(0.66)	(0.02)
$CAR_{(t-240-t-8)}$	−	−0.75*	−1.41***	−1.42***
		(2.06)	(7.00)	(7.99)
Size Quintile 1	?	0.95	0.55	0.52
		(0.72)	(0.25)	(0.24)
Size Quintile 2	?	−0.05	−1.00*	−0.90*
		(0.00)	(1.88)	(1.69)
Size Quintile 4	?	0.23	−0.60	−0.51
		(0.15)	(1.08)	(0.87)
Size Quintile 5	?	0.07	−1.02**	−0.64
		(0.01)	(3.06)	(1.46)

(continued on next page)

TABLE 6 (continued)

	Pred. Sign	CEO Turnover	CFO Turnover	CEO or CFO Turnover
Pseudo-R^2 (%)		18.89	34.27	30.63
Log Likelihood		21.21	43.68	40.34
n		162	162	162

***, **, * Represent p-values < 0.01, 0.05, and 0.10, respectively (p-values are reported as one-tailed when the sign is predicted).

Logistic regressions of variations of model (1) are reported above with short-term CARs to proxy for severity. Chi-square statistics are in parentheses. Pseudo-R^2 is the Nagelkerke (1991) Pseudo-R^2. Sample selection information is detailed in Table 1. We lose 26 observations due to certain missing data on Compustat or CRSP, leaving 162 observations. To approximate the sample proportions we would expect to observe in the population of all restatements, we estimate the logistic regression assigning approximately 3.9 times the weight to the low severity cases and then normalizing the sample size. The dependent variable, *TURNOVER*, is 1 if the executive left the firm in the six months before or six months after the restatement or investigation announcement, whichever is first, and 0 otherwise. $CAR_{(t-7,t+7)}$ is the firm's cumulative abnormal returns from seven trading days prior to the restatement announcement through seven trading days after the announcement. *SUBSIDIARY* is 1 if the restatement occurred in a subsidiary, and 0 otherwise. *MAGNITUDE* is the amount of Net Income overstatement scaled by total assets in the year prior to the restatement announcement. *REVREC* is 1 if the misstatement is partly related to revenue recognition, and 0 otherwise. *ANNUAL* is 1 if the firm restated a 10-K, and 0 if the firm restated only 10-Qs. *CEO_EQUITY* is the fraction of CEO ownership in the year of the restatement. *LEVERAGE* is Debt (#9 + #34) / Assets (#6). *ROA* is operating income before interest and taxes scaled by assets (Compustat #178/#6). $CAR_{(t-240,t-8)}$ is the firm's cumulative abnormal returns from 240 trading days prior to the restatement announcement to 8 trading days prior. Expected returns are the CRSP value-weighted returns inclusive of dividends.

turnover regressions, respectively. These findings suggest that, although short-window CARs are a good proxy for severity, *IRREGULARITY* appears to be more effective in explaining turnover.[32]

Descriptive Review of Irregularities with No Turnover

The high turnover rates that we document are suggestive of boards acting swiftly to remove managers involved in irregularities, but not all parent-level irregularities in our sample lead to the resignation of the CEO and CFO. To understand what factors explain why neither the CEO nor the CFO departs within the 13-month window in 20 percent (16 of 81) of the parent-level irregularity cases, we perform a detailed examination of each of these cases. Our review of these cases is summarized in Table 7.

There are eight cases where either the CEO or CFO left the firm in the 18 months preceding the 13-month window (i.e., from two years to six months prior to the restatement). In these cases, it is likely that the incoming CEO (CFO) was not blamed for the misstatement as he/she was able to attribute the problem to his/her predecessor (current management may in fact have been the one to discover the problem). There are three cases where the CEO or CFO left the firm after the six-month window around the restatement announcement, but the departure occurs around the time the investigation was concluded. These "no turnover" cases are attributable to our use of a shorter turnover window. Finally, there are four cases where it was subsequently concluded

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[32] One potential reason that CARs may be less effective in explaining turnover stems from the measurement error caused by the difficulty identifying the exact announcement date at which to measure the CAR.

TABLE 7

Analysis of *IRREGULARITY* Cases with Neither CEO Nor CFO Turnover in the 13-Month Window around the Restatement Announcement

		Count
Total number of observations classified as intentional and parent-level where neither the CEO nor the CFO left within the 13-month window (−6 to +6 months).		16
Either the CEO or CFO left the firm between 24 and 7 months before the restatement announcement. These are cases where the incoming officers likely attributed the misstatement to their predecessors.	8	
Either the CEO or CFO left the firm at a date later than six months after the restatement but around the time the investigation is concluded.	3	
The investigation (independent internal or external) specifically determined that the misstatement was not intentional.	4	
Total		15
Case of apparent fraud where there was no turnover (Asconi)		1

Summary of Asconi Case:

The Company (Asconi) issued ten million shares during 2003 to the company's CEO and CFO, who together hold over 90 percent of the company's stock. The issuances were originally treated as if they were equity transactions. The SEC conducted a formal investigation and determined that the market value of the ten million shares of Common Stock issued during 2003 should have been charged against the Company's income statement as compensation. The investigation remains unresolved, but Asconi completed a reverse stock split in 2006 that reduced the number of shareholders to less than 300 and thus relieves them from public filing requirements.

either from the independent investigation or the SEC investigation that no intentional GAAP violations actually occurred. This leaves only one case, Asconi Corp., where it appears that an irregularity occurred but neither the CFO nor the CEO was terminated. However, Asconi's CEO and CFO together hold over 90 percent of the company's stock, which makes them fairly atypical for a public firm. We conclude from this analysis that boards rarely fail to discipline senior management when intentional misstatements are discovered.

Changes in Mix of Errors versus Irregularities over Time

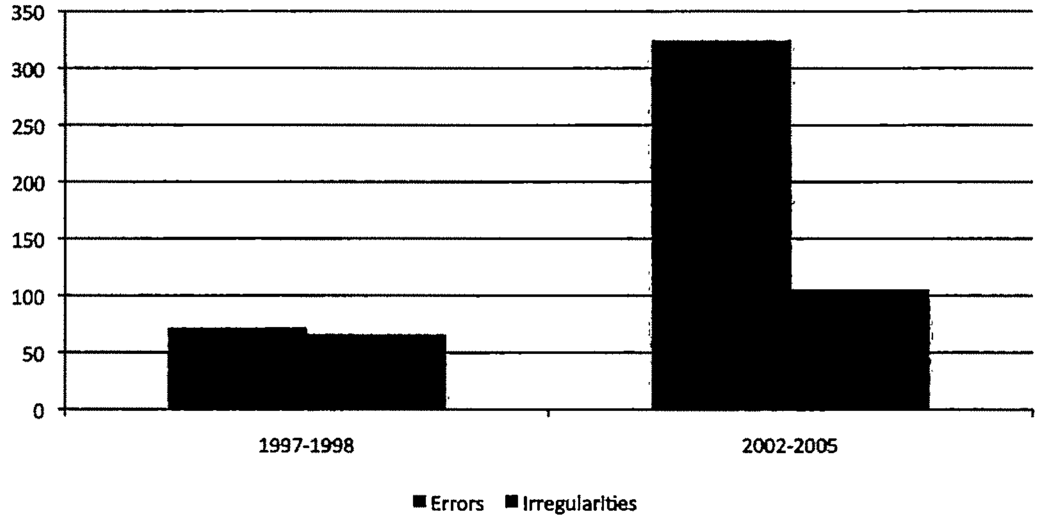
In this section, we examine whether changes in the regulatory environment resulting from accounting scandals occurring in 2001 increased the likelihood that boards will terminate executives when they disclose accounting restatements. This analysis provides an opportunity to demonstrate the importance of partitioning errors and irregularities, since the mix of errors versus irregularities may change over time and failure to separate irregularities would lead to incorrect inferences.

To demonstrate this potential problem, we construct a second sample from the GAO database (2003) and include all restatements between January 1, 1997 and December 31, 1998. We select this time period because it provides a clean window before the accounting scandals and subsequent regulation and is comparable to other recent research on restatements (e.g., Desai et al. 2006). The 1997–1998 sample consists of 194 unique firms as identified by the GAO database. Of these, we exclude 16 firms in the financial services industry (SIC codes 6000–6999) and 29 firms that are not publicly traded or otherwise have missing data. We drop an additional 13 observations where further investigation reveals that the restatement had no income impact, pertained only to the re-presentation of now-discontinued operations, or was announced contemporaneously with a merger. This results in a final sample of 136 restatements for 1997–1998 sample.

Figure 5 compares the proportion of *ERRORS* versus *IRREGULARITIES* in the 1997–1998 sample to that of the 2002–2005 sample. The proportion of *IRREGULARITIES* went from 48 percent in the 1997–1998 GAO sample down to 24 percent in our 2002–2005 sample.[33] The reason for this change is primarily due to the substantial increase in more innocuous restatements that occurred after the Sarbanes-Oxley Act and implementation of Section 404. One obvious implication of this change in mix is that incorrect inferences about the causes and consequences of restatements over time are likely to be made if intentional restatements are not distinguished from errors.

We compare turnover rates in the 1997–1998 sample to the 2002–2005 sample to see if the increased focus on governance has led to a corresponding increase in executive turnovers around restatements. Table 8 reports logistic regression results estimating model (1) with the addition of

FIGURE 5
Errors versus Irregularities



The 1997–1998 sample consists of 194 unique as identified by the GAO database. From these, we exclude 16 firms in the financial services industry (SIC codes 6000–6999) and 29 firms that are not publicly traded or otherwise have missing data. We drop an additional 11 observations where further investigation reveals that the restatement had no income impact, pertained only to the re-presentation of now-discontinued operations, or was announced contemporaneously with a merger. This results in a final sample of 138 restatements for 1997–1998 sample. The sample consists of 72 errors and 66 irregularities. See Table 1 for a description of the 2002–2005 sample.

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[33] We classify 2002–2005 restatements in the now-released updated GAO sample and find a similar ratio of irregularities (21 percent) to errors (79 percent), which suggests that the high prevalence of restatements caused by errors in our sample is not a function of different sample selection procedures.

TABLE 8

Logistic Regression *PRE* versus *POST* Analysis

$$TURNOVER = \beta_0 + \beta_1 IRREGULARITY + \beta_2 POST + \beta_3 SUBSIDIARY + \beta_4 ANNUAL + \beta_5 CEO\_EQUITY + \beta_6 LEVERAGE + \beta_7 ROA + \beta_8 CAR_{(t-240-t-8)} + \varepsilon$$

	Pred. Sign	CEO Turnover		CFO Turnover		CEO or CFO Turnover	
		(1)	(2)	(3)	(4)	(5)	(6)
Intercept		−1.04*	−1.89***	−0.37	−1.28**	0.29	−0.49
		(4.24)	(11.59)	(0.63)	(5.76)	(0.38)	(0.83)
IRREGULARITY	+		1.95***		2.4***		2.65***
			(37.22)		(48.24)		(54.28)
POST	?	−0.54*	−0.19	−0.41	−0.02	−0.8**	−0.46
		(2.14)	(0.23)	(1.37)	(0.00)	(5.07)	(1.28)
SUBSIDIARY	−	−0.39	−0.84**	−0.49	−1.12***	−0.62*	−1.42***
		(0.87)	(3.51)	(1.54)	(5.93)	(2.62)	(9.14)
ANNUAL	+	0.49*	0.3	0.3	0.14	0.39	0.2
		(1.93)	(0.61)	(0.79)	(0.13)	(1.34)	(0.26)
CEO_EQUITY	−	−0.03*	−0.04**	−0.01	−0.02	−0.01	−0.02*
		(2.42)	(3.04)	(0.42)	(1.21)	(0.82)	(2.21)
LEVERAGE	+	−0.09	−0.17	1.06**	1.25**	0.79*	0.91*
		(0.02)	(0.07)	(3.69)	(4.31)	(2.05)	(2.24)
ROA	−	−1.68**	−0.91	−1.06*	−0.11	−2.03***	−0.96
		(5.15)	(1.37)	(2.18)	(0.02)	(6.71)	(1.45)
$CAR_{(t-240-t-8)}$	−	−0.81***	−0.44*	−1.17***	−0.84***	−1.15***	−0.81***
		(6.08)	(1.79)	(13.49)	(6.85)	(13.93)	(6.11)
Pseudo-R^2 (%)		14.07	30.97	21.57	42.43	24.42	47.53
Log Likelihood		30.32	71.34	50.83	109.9	59.8	130.19
n		298	298	298	298	298	298

***, **, * Represent p-values < 0.01, 0.05, and 0.10, respectively (p-values are reported as one-tailed when the sign is predicted).

Logistic regressions of variations of model (1) are reported above. Chi-square statistics are in parentheses. Pseudo-R^2 is the Nagelkerke (1991) Pseudo-R^2. Sample selection information is detailed in Table 1. We include the 126 firms from the pre-period and the 172 firms from the post-period that are not missing data. As previously discussed, in the post-period we assign approximately 3.9 times the weight to the low severity cases and then normalizing the sample size. The dependent variable, *TURNOVER*, is 1 if the executive left the firm in the six months before or six months after the restatement or investigation announcement, whichever is first, and 0 otherwise. *IRREGULARITY* is 1 if the restatement announcement explicitly refers to an irregularity as the reason for the restatement or discloses either an SEC investigation or an independent board investigation, and 0 otherwise. *POST* is 0 if the restatement occurred in 1997 or 1998, and 1 if the restatement occurred between 2002 and 2005. *SUBSIDIARY* is 1 if the restatement occurred in a subsidiary, and 0 otherwise. *ANNUAL* is 1 if the firm restated a 10-K, and 0 if the firm restated only 10-Qs. *CEO_EQUITY* is the fraction of CEO ownership in the year of the restatement. In cases where we are unable to obtain ownership information in the year of the restatement, we use the prior year. *LEVERAGE* is Debt (#9 + #34) / Assets (#6). *ROA* is operating income before interest and taxes scaled by assets (Compustat #178/#6). $CAR_{(t-240-t-8)}$ is the firm's cumulative abnormal returns from 240 trading days prior to the restatement announcement to 8 trading days prior, and expected returns are the CRSP value-weighted returns inclusive of dividends. Size quintile controls are included in the regression but not reported in the tables (for brevity).

an indicator variable, POST, which is equal to 1 if the restatement is from our 2002–2005 sample, and 0 if it is from our 1997–1998 sample. For ease of exposition, we exclude the interaction term between *IRREGULARITY* and *SUBSIDIARY*. Size quintile controls are included in the regression but not reported in the table for brevity. Column (1) reports results with CEO Turnover as the dependent variable and *IRREGULARITY* left out of the regression. Without *IRREGULARITY*, the coefficient on *POST* is negative and significant ($p < 0.01$), suggesting the turnover rates around restatements actually declined across the time periods. Similar results are obtained in column (5) for CEO/CFO turnover, although there is no evidence of a change in CFO turnover in column (3). However, when the *IRREGULARITY* indicator variable is added to the regressions, *POST* is insignificant in the CEO turnover (2), CFO turnover (4), and combined CEO/CFO turnover (6) regressions.

Results using only restatements classified as irregularities are presented in Table 9. The coefficient on *POST* is insignificant across all three regressions when only *IRREGULARITY* cases are used. This suggests that there is no evidence that turnover rates for irregularities have changed significantly across the two sample periods. Given the extremely high turnover rates in the 2002–2005 sample period, we do not interpret this as the continuance of ineffective governance even after additional regulation was implemented. Instead, we interpret this as indicating that boards continue to be effective at taking action in response to irregularities. This analysis emphasizes how important the irregularity/error partition can be to inferences based on mixed samples.

V. CONCLUDING COMMENTS

Research on restatements has grown significantly over the past several years. The vast majority of this research relies on the GAO databases that include a mix of both *errors* and *irregularities*. We propose a straightforward procedure for distinguishing errors from irregularities in a large sample of restatements. We classify restatements as likely being caused by irregularities if the firm explicitly uses variants of "fraud" or "irregularity" in connection with the restatement, if there is a related SEC or Department of Justice (DOJ) investigation, or if there is an (non-SEC) independent investigation into the misstatement. A restatement not meeting any of these criteria is classified as involving only an error. This classification scheme is based on prior research, the reading of numerous restatement disclosures, and on guidance boards receive from the auditors, the SEC, and lawyers specializing in restatement litigation.

We validate our measure in several ways. First, we find that announcement CARs for the irregularities sample are significantly more negative than for the error sample. As a second validity test, we compare the frequency of securities class action lawsuits in the irregularities sample to the errors sample. We find in instances where a class action suit is likely (e.g., large market cap, parent-level irregularity, negative announcement returns) the vast majority of irregularities do result in class action lawsuits. Further, we find only one occurrence of a restatement in our error sample that resulted in a class action lawsuit. Both the CARs and the class action patterns suggest that our classification procedure yields a meaningful partition of restatements.

To demonstrate the importance of partitioning errors versus irregularities, we re-examine the relation between restatements and CEO/CFO turnover studied in earlier research. As expected, when we partition the restatements, turnover rates are much higher for irregularities than for errors. In our 2002–2005 sample, in the 13 months surrounding the restatements (six months before to six months after) 49 percent (64 percent) of firms experience turnover of their CEOs (CFOs) in the irregularities sample, but only 8 percent (12 percent) in the errors sample. After excluding subsidiary-level restatements and expanding the turnover window to four years (two years before and two years after) to be more consistent with prior research, the (untabulated) turnover rates for irregularity restatements are 67 percent for CEOs and 85 percent for CFOs. In

TABLE 9 (continued)

parentheses. Pseudo-R^2 is the Nagelkerke (1991) Pseudo-R^2. Sample selection information is detailed in Table 1. The dependent variable, *TURNOVER*, is 1 if the executive left the firm in the six months before or six months after the restatement or investigation announcement, whichever is first, and 0 otherwise. *SUBSIDIARY* is 1 if the restatement occurred in a subsidiary, and 0 otherwise. *POST* is 0 if the restatement occurred in 1997 or 1998, and 1 if the restatement occurred between 2002 and 2005. *ANNUAL* is 1 if the firm restated a 10-K, and 0 if the firm restated only 10-Qs. *CEO_EQUITY* is the fraction of CEO ownership in the year of the restatement. *LEVERAGE* is Debt (#9 + #34) / Assets (#6). *ROA* is operating income before interest and taxes scaled by assets (Compustat #178/#6). $CAR_{(t-240 \to t-8)}$ is the firm's cumulative abnormal returns from 240 trading days prior to the restatement announcement to 8 trading days prior, and expected returns are the CRSP value-weighted returns inclusive of dividends.

91 percent of these irregularity cases, either the CEO or CFO leaves the firm. This evidence suggests that the relatively low turnover rates documented in past research are due largely to the inclusion of unintentional errors in prior restatement samples.

Finally, we demonstrate that conditioning on restatement type is critical to interpreting changes in turnover rates over time. Without controlling for errors versus irregularities, we obtain the counter-intuitive result that turnover rates around restatements have declined in 2002–2005 as compared to 1997–1998. However, when examining only cases where turnover would be expected (irregularities), we find no evidence of either a decline or an increase in turnover rates for irregularities across the two time periods.

For researchers, our findings suggest that the power of tests on the consequences of restatements (e.g., turnover, market reaction, etc.) can be greatly enhanced by classifying restatements as either errors or irregularities and, in the specific case of restatements and turnover, focusing on relatively shorter turnover windows. Data sources such as the GAO sample mix together both intentional (irregularities) and unintentional (errors) violations of GAAP, which are substantially different in perceived severity. Our procedure for identifying irregularities appears quite effective at distinguishing between irregularities and errors and is relatively easy to implement in future research utilizing a restatement setting.

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Do Individual Investors Cause Post-Earnings Announcement Drift? Direct Evidence from Personal Trades

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ABSTRACT: This study tests whether naïve trading by individual investors, or some class of individual investors, causes post-earnings announcement drift (PEAD). Inconsistent with the individual trading hypothesis, individual investor trading fails to subsume any of the power of extreme earnings surprises to predict future abnormal returns. Moreover, individuals are significant net buyers after *both* negative and positive extreme earnings surprises, consistent with an attention effect, but not with their trades causing PEAD. Finally, we find no indication that trading by individuals explains the concentration of drift at subsequent earnings announcement dates.

Keywords: *post-earnings announcement drift; market efficiency; individual investors; investor sophistication.*

Data Availability: *All data other than that provided by an anonymous discount broker are publicly available.*

JEL Classifications: *G14; M41.*

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I. INTRODUCTION

Post-earnings announcement drift (PEAD) is the tendency for stocks to earn positive average abnormal returns in the three quarters subsequent to extreme positive earnings surprises and, more strongly, to earn negative average abnormal returns in the three quarters subsequent to extreme negative earnings surprises. This phenomenon is widely regarded as a puzzle from the perspective of the efficient markets hypothesis.

Bernard and Thomas (1990) suggest that investor naïveté about the time-series properties of earnings may drive drift.[1] Several studies suggest that institutional investors are more sophisticated traders than are individual investors (Hand 1990; Lee et al. 1991; Walther 1997; Grinblatt and Keloharju 2000; Balsam et al. 2002; Bonner et al. 2003; Asthana et al. 2004; De Franco et al. 2006; Mikhail et al. 2007), and some suggest that PEAD may result from the trading activity of individuals. We call this proposition the *individual trading hypothesis*.

Bartov et al. (2000) provide a degree of support for this argument. They report that under some specifications, PEAD is strongest in firms with low institutional shareholdings (and thus, high individual shareholdings), but that the results are mixed. Because of this, the authors point out that their results do not provide strong evidence about whether individual investors cause PEAD. More recent evidence from changes in institutional ownership is mixed as to whether institutions are sophisticated arbitrageurs. Although Burch and Swaminathan (2003) document that institutional investors buy after both positive and negative earnings surprises, Ke and Ramalingegowda (2005) report that some types of "transient" institutions arbitrage drift.[2]

Evidence from large versus small trades made after earnings announcements is also mixed as to whether naïve individual investors cause drift. Results in Bhattacharya (2001) and in Battalio and Mendenhall (2005) are consistent with individuals causing PEAD[3] but those in Shanthikumar

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[1] There is debate as to the nature of bias that might induce naïve investors to trade in a way that generates drift. Alternatives to the type of naïveté proposed by Bernard and Thomas (1990) are proposed in subsequent research (Ball and Bartov 1996; Jacob et al. 2000). Barberis et al. (1998) and Daniel et al. (1998) provide formal models in which PEAD can arise as an under-reaction to earnings, owing to psychological biases such as overconfidence and conservatism. There is debate in the empirical literature about whether PEAD reflects a rational risk premium, a simple tendency for investors to underreact to earnings news, or a more complex intertemporal pattern of short-term under-reaction and long-term over-reaction to earnings (e.g., Lakonishok et al. 1994; Bernard et al. 1997; Dechow and Sloan 1997; Lee and Swaminathan 2000; Daniel and Titman 2006).

[2] Some recent papers (e.g., Musto 1999; Griffin et al. 2003; Jackson 2003a, 2003b; Coval et al. 2005; Dasgupta et al. 2006; Kaniel et al. 2008) suggest that there is no simple dichotomy between naïve individuals and smart institutions. For example, there is evidence that some institutional investors engage in trades for "window-dressing" purposes (Musto 1999), and some individual investors are able to achieve persistent high returns relative to standard benchmarks (Coval et al. 2005). Furthermore, institutions chase daily trends apparently without profiting thereby (Griffin et al. 2003). Jackson (2003a) provides evidence suggesting that institutions, rather than individuals, make non-fundamental based trades, and Jackson (2003b) reports that the net trades of brokerage clients in Australia positively forecast future returns. Dasgupta et al. (2006) provide evidence that stocks which were purchased by several institutional investors (perhaps owing to "herding") over the preceding five quarters earn low returns, and that those that were sold earn high returns. With respect to PEAD itself, Burch and Swaminathan (2003) provide some evidence consistent with institutions in the aggregate driving poor returns after negative earnings surprises, and Ke and Ramalingegowda (2005) provide evidence that "transient" institutions act as earnings contrarians, selling after positive surprises and buying after negative ones.

[3] Lee (1992) provides mixed evidence in this regard. He examines inferred-signed trades of investors for a sample of approximately 230 firms during 1988. He finds that small trades tend to be inferred-buys for more than two days after both favorable and unfavorable earnings surprises relative to analyst forecasts. Since individual investors tend to make smaller trades, Lee (1992) suggests that his findings are consistent with earnings announcements drawing the attention of individual investors to the stock. Bhattacharya (2001) provides evidence that the volume of small trades, but not large trades, is associated with the magnitude of random walk earnings surprises, suggesting that investors who make small trades (presumably less sophisticated investors) may cause PEAD. Battalio and Mendenhall (2005) provide evidence that large trades (made presumably by more sophisticated investors) respond more strongly to surprises relative to analyst forecasts, whereas small trades respond more strongly to surprises relative to a seasonal random walk model.

(2004) are not.[4] However, as the authors of these studies recognize, trade size is not necessarily a good indicator of whether the trader is an individual or institution, nor whether the trader is sophisticated. To reduce the price impact of their trades, sophisticated investors split orders and make smaller trades when they disagree with the market price (Barclay and Warner 1993; Bernhard and Hughson 1997; Diether et al. 2007).[5] Furthermore, Campbell et al. (2005) provide evidence that institutions tend to make both very large and very small trades, with individuals tending to make intermediate-sized trades.

In this paper, we offer direct tests of the individual trading hypothesis by examining actual individual investor trades following earnings announcements (rather than relying on trade size to proxy for trader identity). We examine all trades made by a random sample of individual investors through a major discount brokerage from 1991 through 1996. Under the individual trading hypothesis, trading by individual investors impedes a full price response after an earnings announcement, leading to under-reaction and PEAD. We therefore examine whether individual trading after earnings announcements subsumes some of the ability of earnings to predict subsequent abnormal returns. We also test whether, as called for by the individual trading hypothesis, individual investors (as a group or in relevant subcategories) trade as contrarians to earnings surprises. Finally, we examine individual trading in the days surrounding subsequent quarterly earnings announcements, to see whether these trades are consistent with individuals driving the concentration of drift at later earnings announcement dates.

Our paper differs from extant studies of PEAD because we examine the relation between trading behavior and subsequent returns, along with the relation between trading behavior and earnings surprises. Since PEAD is a returns and an earnings phenomenon, doing so allows us to speak more directly to the individual trading hypothesis. Our paper also differs from past studies because we examine *actual daily signed trades* made by individual investors after earnings surprises while past studies have used indirect methods.[6]

A key advantage of using data on actual daily signed trades is that it allows us to incorporate the daily timing of the individual investor trades as well as whether the trade is in the same direction as the earnings surprise (i.e., net buying after good news and net selling after bad news) or in opposition to it. Furthermore, our data provides us with two proxies for individual investor sophistication—capital invested and total trading activity. We use these proxies to test whether the least sophisticated investors—those with relatively little capital invested with the discount broker and/or those with relatively little trading experience—drive PEAD.

Since markets must clear, evidence from institutional trading is complementary with and potentially informative about individual trading. However, past evidence on institutional trading does not capture the information provided by our sample and method. Specifically, the CDA-Spectrum position data, used in institutional investor studies, is derived from quarterly SEC 13f filings. This sample is not ideal for testing the individual trading hypothesis for at least two reasons. First, individual positions are not just the inverse of institutional positions as inferred from 13f filings.[7] Second, quarterly net position changes are aggregates of trades made at different times throughout the quarter.

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[4] Shanthikumar (2004) documents subtle patterns in the behavior of large and small trades made in relation to earnings surprises measured relative to analyst forecasts versus a seasonal random walk model, and concludes that there is some evidence of under-reaction in *both* large and small trades.

[5] Barclay and Warner (1993) find that medium-sized trades affect price more than do large- or small-sized trades. Diether et al. (2007) find that small sell trades predict negative future returns, while medium and large sell trades do not. Both kinds of evidence suggest that investor sophistication is not monotonically increasing with trade size.

[6] These include examining the fraction of shares held by institutions, inferring institutional trades from quarterly changes in stockholdings, inferring the identity of investors (i.e., individual versus institutions) by trade size, inferring probabilistically the direction of the trade (i.e., buy versus sell) from microstructure data, and/or testing the properties of unsigned trading volume.

A benefit of our data is that it allows us to distinguish trades made one day before an earnings announcement from those made one day after—cases that have completely different implications for whether traders are driving drift. Empirically, PEAD has a sharp conditioning date (i.e., the earnings announcement date) and the effect is concentrated on particular days after the initial announcement of extreme earnings (i.e., around later earnings announcement dates). The use of daily data permits us to test whether investors purchase the day before the subsequent earnings announcement and reverse their position a few days thereafter, but with quarterly positions data, such behavior is invisible.

If individuals drive PEAD, then individual net sells after good earnings news (which generate underpricing) should predict high subsequent stock returns, and individual net buys after bad news (which generate overpricing) should predict low subsequent returns. Therefore, we examine the relation between earnings surprises, individual trading, and subsequent stock returns. Furthermore, if individual trading is a source of the relation between earnings surprises and subsequent returns, then the predictive power of individual trades should remain even after controlling for the earnings surprise. Most directly of all, if individual trading drives PEAD, then individual trading after earnings surprises should subsume part or all of the ability of the earnings surprise to predict subsequent returns. Thus, our paper differs from prior work in directly examining whether trading by individual investors subsumes the ability of earnings surprise to predict subsequent returns.

If individual investors are naïve with respect to earnings surprises, then we expect to see significant net buying after negative earnings surprises and significant net selling after positive earnings surprises. Furthermore, given the evidence of stronger downward PEAD than upward PEAD, the tendency of naïve individuals to buy after negative surprises should be stronger than the tendency of naïve individuals to sell after positive surprises.

We also perform tests focusing on trades made during the days prior to the subsequent quarterly earnings announcement, where PEAD is strongest (Bernard and Thomas 1990). Given a positive (negative) earnings surprise, sophisticated investors should buy (sell) shares in the days prior to the next quarterly earnings announcement. For drift to exist despite arbitrage by sophisticated investors, naïve investors must be trading in the opposite direction, impeding the rapid adjustment of prices. Thus, if individual investors are naïve, then they will sell (buy) just prior to the earnings announcement immediately following a favorable (unfavorable) earnings announcement.

Results of our tests suggest that individual investors do *not* cause PEAD. We base this conclusion on three kinds of evidence. First, controlling for net trading by individuals does not reduce the ability of extreme earnings surprises to predict subsequent returns. Second, rather than trading in opposition to earnings surprises, individuals are significant net purchasers after both good and bad earnings news.[8] This pattern also holds for every investor class, suggesting that even the least sophisticated investors (i.e., those with low invested capital and low trading activity) are not driving drift. Third, when we measure the extent to which, conditional on an earnings surprise at a given date, individuals make abnormal trades in the days just prior to or just after the *subsequent* quarterly earnings announcement, our results are not consistent with the trading pattern (discussed earlier and in Section II) predicted by the hypothesis that individual investor trading causes the concentration of PEAD at subsequent earnings announcement dates.

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[7] 13f data on institutions includes only institutions with greater than $100 million invested in equity securities, and even for these institutions, only positions of at least 10,000 shares or $200,000 need be disclosed. Thus, 13f data cannot resolve which other categories of investors—individuals, smaller institutions, or large institutions making smaller trades—help drive drift.

[8] Interestingly, we find that the greater the absolute value of the earnings surprise, the greater the quantity of shares purchased, but that the direction of trading is unrelated to the direction of the news. This is consistent with trading by individuals being influenced by a news attention effect (Barber and Odean 2008).

The remainder of this paper is structured as follows. Section II explains in more detail how trading by individual investors could induce PEAD. Section III contains a description of the data, sample selection criteria, variable definitions, and descriptive statistics. In Section IV, we examine the relation between individual trading, earnings surprises, and subsequent stock returns. Section V provides evidence on individual investor trading following extreme earnings surprises, and Section VI examines individual trading, conditional on an extreme earnings surprise, around the subsequent quarterly earnings announcement. Section VII concludes.

II. NAÏVE TRADING AND POST-EARNINGS ANNOUNCEMENT DRIFT

Testing whether individuals are the source of drift is useful only if we cannot exclude this possibility on *a priori* conceptual grounds. A possible argument against individuals driving drift is that institutions are big traders and therefore dominate price-setting. However, during the last year of our sample period, individuals held 48 percent of the market value of common stock (Securities Industry Association 2002). Thus, there is reason to expect individuals (as well as institutions) to play a significant role in price-setting.

One could also argue that drift could not represent a market inefficiency because if naïve trading were to induce such a pattern of mispricing, smart arbitrageurs would find it profitable to trade to exploit it. This would tend to attenuate the pattern. However, a literature in behavioral finance and accounting argues that despite arbitrage by sophisticated investors, the behavior of imperfectly rational investors can induce mispricing (such as PEAD), and under some circumstances, mispricing can persist.[9] If naive investors are subject to common misperceptions, then, in equilibrium, these misperceptions influence price by an amount that depends on the relative sizes and risk tolerances of different investor groups (and since the individual trading hypothesis requires common misperceptions, if individual investors drive PEAD, then we expect to see evidence of misperceptions within our sample of individual investor trades). Evidence exists that, at least in some cases, the beliefs of unsophisticated investors influence security prices.[10] Furthermore, if sophisticated investors are risk-averse, then the degree to which they arbitrage mispricing will be limited. Finally, Lamont and Thaler (2003) discuss how limits to short-selling can prevent prices from adjusting to reflect the views of sophisticated investors.

Some authors suggest that PEAD represents a market inefficiency (e.g., Bernard and Thomas 1989, 1990; Bernard et al. 1997; Fama 1998; Mendenhall 2004), while others suggest that PEAD may reflect estimation issues such as a return benchmark not commensurate with risk (e.g., Ball 1992). Given existing theory and evidence, the hypothesis that PEAD is a market inefficiency resulting from individual investor trading deserves to be tested.

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[9] See, for example, the models and surveys in DeLong et al. (1991), Kandel and Pearson (1995), Shleifer and Vishny (1997), Daniel et al. (1998), Fischer and Verrecchia (1999), Lee (2001), and Hirshleifer and Teoh (2003). These models indicate that irrational investors can influence price in the short run. Specifically, if irrational investors have non-negligible risk-bearing capacity, in general, then they affect price. Moreover, whether rational investors will earn high profits at the expense of irrational investors, so that in the long run, the influence of irrational investors is eliminated, depends on the model specification.

[10] For example, confusion by investors over ticker symbols can cause short-run price reactions to news about unrelated firms (Rashes 2001). Moreover, during the Internet boom, relative mispricing between parent firms and sexy high-tech divisions existed (Lamont and Thaler 2003).

A possible limitation of using our sample to test the individual trading hypothesis is that it contains trades made by a random sample of investors at a single major discount brokerage. Whether this allows for an unbiased test depends on whether the investors at this brokerage are representative of individual investors as a whole. Several kinds of evidence suggest that the sample is broadly representative. First, early in the sample period, this brokerage had more than 1.25 million clients while the total number of individuals with direct share ownership of U.S. firms in the closest comparison year was 29.2 million (see "Shareownership 2000," NYSE, http://www.nyse.com/pdfs/shareho.pdf). Therefore, the brokerage represented approximately 4 percent of the population of individual shareholders. Second, we have no reason to expect that the individuals dealing with this brokerage are unusually naïve or sophisticated, relative to those dealing with other brokerages. Rather, the sample is a broad cross-section that includes a mixture of both traditional and online traders. Third, Ivkovich et al. (2005) document that patterns of stock sales by investors in our sample correspond well with general data on investor stock sales reported on income tax returns, and Ivkovich et al. (2008) provide evidence that the number of stocks and the total stock portfolio value held by households in our sample correspond well with data from the Federal Reserve Board's Survey of Consumer Finances on the stock holdings in the general U.S. population. Similarly, Barber and Odean (2000), Dhar and Kumar (2002), and Ivkovich et al. (2008) provide other tests which support the representativeness (in relation to demographic characteristics) of the sample for U.S. investors in general.[11]

There are, however, some differences between customers of the discount brokerage that we examine and other individual investors. For example, customers of full-service brokerages are likely to receive more advice about what stocks to choose, which could steer them away from naïve trades. Furthermore, our sample is likely to contain relatively few extremely wealthy individual investors. Again, we expect such investors to be relatively sophisticated and to have the benefit of professional advice. These considerations suggest that our sample is comprised of the subset of individual investors who would be most likely to drive drift. However, for agency reasons, full-service brokerages might encourage investors to make naïve active trades to boost commissions (a practice known as "churning"). Moreover, it is possible that overconfidence or agency problems on the part of these full-service brokerages might cause investors to make systematic errors in response to earnings announcements. Thus, we cannot rule out the possibility that drift is driven by some groups of individual investors that are not part of our sample. What we can be sure of is that the investors within our sample *do* make other systematic trading errors, and that there is ample power within our data set to identify these systematic trading errors (see, for example, Odean 1999; Barber and Odean 2000).

Even taking as given that individual investors can affect prices, it could be argued that a sample that includes only a subset of individual investors may not include the "marginal investors" who determine prices. However, in models of securities pricing that explicitly analyze how prices are determined in equilibrium through the market-clearing condition, prices reflect a weighted average of the beliefs of different traders, where the weights reflect the risk-bearing capacities of the different traders (Kandel and Pearson 1995; Hirshleifer and Teoh 2003). Thus, in these models, there is no single decisive group of "marginal investors"—the beliefs of every investor influence price.[12] Furthermore, we believe that the conclusion to be drawn from the evidence that we present is independent of whether one adheres to a "marginal investor" perspective or to the weighted average perspective. Under either perspective, if our sample is representative, then our tests tell us about the behavior of individual investors as a whole, and if our sample is unrepresentative, then our tests do not speak to whether some other group of individual investors drives drift.

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[11] For example, Dhar and Kumar (2002) and Ivkovich et al. (2008) verify that the portfolio size in our overall sample and for different investor age groups is very similar to that for comparable U.S. investors in general.

[12] In general, in microeconomics, the price of a commodity depends on the demand curves of all traders, not just a "marginal" subset. Similarly, models of security trading show that the demand curves of all investors play a role in price determination. Hirshleifer and Teoh (2003) analyze and discuss the "marginal investor" issue in an accounting context.

Post-earnings announcement drift is typically characterized as an under-reaction to earnings news. Bernard and Thomas (1990) show that seasonal quarterly earnings changes are positively serially correlated. That is, after a positive (negative) earnings surprise, subsequent earnings surprises tend to be predictably positive (negative). Furthermore, abnormal stock returns subsequent to earnings surprises are predictable. While stock prices generally increase (decrease) upon the announcement of good (bad) earnings news, they do not seem to increase (decrease) enough. In consequence, returns are, on average, abnormally high (low) for the next three quarters (but at a decaying rate).

A group of investors that drives PEAD would trade in a way that opposes a full and rational stock price adjustment in response to earnings surprises. Thus, after favorable (unfavorable) earnings news, these individuals would, on average, sell (buy) stock. In other words, they could be contrarian with respect to current earnings news. This suggests a simple test of whether individual investors cause PEAD: test whether individuals, on average, buy after extreme negative earnings surprises and sell after extreme positive earnings surprises.[13]

Furthermore, if individual trading causes prices to underreact to earnings news (which manifests as PEAD), then net purchases by individuals must be related to subsequent abnormal stock returns. Thus, the individual trading hypothesis predicts that individual net selling generates underpricing, and should therefore predict high subsequent stock returns, and that individual net buying generates overpricing, and should therefore predict low subsequent stock returns.

Finally, if trading by individual investors is a source of the relation between extreme earnings surprises and subsequent returns, then in those cases where positive (negative) earnings surprises are followed by relatively little net individual investor selling (buying), there should be relatively little upward (downward) drift. Similarly, drift should be stronger in those cases where individuals trade in a more strongly contrarian fashion to the earnings surprise. Thus, individual net purchases should largely or even completely subsume the ability of the earnings surprise to predict subsequent returns. This implication offers a direct and powerful test of the individual trading hypothesis.

Past empirical literature documents that after an extreme earnings surprise, drift is disproportionately concentrated in the days after each of the next two quarterly earnings announcements, and that there is a modest but significant reversal in the days following the fourth subsequent announcement (Bernard and Thomas 1989, 1990). If naïve investors are driving drift, then such a pattern would be apparent in trading by naïve investors around subsequent earnings announcements.

Specifically, conditional on an initial earnings surprise, naïve and sophisticated investors would differ in their assessments of fundamental value. For example, after a favorable announcement, sophisticated investors would believe that price is too low, and their purchases would drive the price higher. Naïve traders would believe that the price has moved up too much, and would therefore tend to sell. During the subsequent quarter, newly arriving public information may not suffice to resolve the gap between naïve and sophisticated expectations. If not, then before the next quarterly earnings announcement, sophisticated traders would buy and naïve traders would sell. Thus, selling by naïve traders would offset the pressure of rational arbitrage trading, preventing the price from adjusting upward sufficiently prior to the next earnings announcement. On average, subsequent earnings would be higher than expected by naïve traders, leading to an abnormally high average return on the earnings announcement date.

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[13] Such a test assumes that the benchmark against which to measure buying or selling pressure on price is zero. For markets to clear, the average net trade by investors after an earnings surprise is zero. Thus, the deviation in a group's net purchases from zero is a measure of the degree to which trading by that group differs from trading by other groups. After an earnings surprise, net purchases by a group creates upward price pressure, and net sales creates downward price pressure. Of course, there are rational models with heterogeneous investors that can accommodate net purchases or sales by individual investors after earnings surprises. Thus, the more general premise of our basic trading tests is that any pressure toward mispricing exerted by individual investors is positively correlated with their net purchases. This rules out the possibility that when individuals are buying, they are on average exerting downward price pressure (toward underpricing), and when they are selling, they are exerting upward price pressure (toward overpricing). The premise needed for our return prediction tests is even milder—if trading by individuals is driving drift, then the price pressure they exert is manifested observably in their trades. We discuss this issue further in the concluding section.

If PEAD represents a market inefficiency, then sophisticated investors can exploit this pattern near subsequent earnings announcements by using a dynamic trading strategy. For example, after a positive earnings surprise, investors can earn high returns by buying shares a few days prior to the next quarterly earnings announcement and partly unwinding their positions in the days following the announcement. Such a strategy offers a favorable balance between risk and expected return.[14,15] As discussed above, naïve investors incorrectly believe it is profitable to take the opposite sides of these trades. When there is a negative earnings surprise, sophisticated investors should do the reverse, selling just before the subsequent earnings announcement. In either case, sophisticated trading tends to accelerate the adjustment of prices.

If unopposed by the trades of naïve investors, then such arbitrage would eliminate the concentration of PEAD at the subsequent earnings announcement dates. However, if naïve investors trade in the opposite direction (further delaying price adjustment), then the concentration of drift remains. Thus, if individual investors are naïve, then, conditional on a favorable (unfavorable) earnings announcement, they will sell (buy) just prior to the next quarterly earnings announcement. These predictions have not hitherto been tested.

III. TRANSACTION DATA, SAMPLE SELECTION, VARIABLE DEFINITIONS, AND DESCRIPTIVE STATISTICS

Transaction Data

The data used in this study consists of trades made by a random sample of 78,000 households with 158,034 accounts at a large discount brokerage. The brokerage made 3,075,797 trades on behalf of these households between January 1991 and December 1996 inclusive. 1,969,747 of these trades involve common stock, while the remainder involves mutual fund shares, bonds, and other securities. The sample is, by construction, a random sample from the population of households with accounts at the brokerage, and is drawn independently of all other variables. Therefore, the sample accurately and without bias represents the full set of individual investors at this brokerage firm. The full set is comprised of approximately 1.25 million households, so our sample represents a set of investors about 20 times as large as the sample itself.

We classify the households in our sample as *actively trading investors* (6,000 households), *high-capital investors* (12,000 households), and *general investors* (60,000 households).[16] Any investor who conducts more than 48 trades in a year is classified as actively trading, investors who

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[14] While the risk that is related to earnings announcements is greater at the time of the earnings announcements, the expected return is also greater around subsequent earnings announcements. Concentrating trades near the dates of earnings announcements reduces extraneous risk that is unrelated to these announcements.

[15] Even investors who do not trade actively to exploit drift can benefit in the quarters following a positive earnings surprise by advancing any planned purchases from a few days after to a few days before a subsequent earnings announcement, and by deferring any planned sales from a few days before to a few days after a subsequent earnings announcement.

[16] Here, we follow the brokerage's classification scheme, but change the brokerage's label from affluent to high-capital investors to more accurately reflect the contents of this category.

are not classified as actively trading and have more than $100,000 of invested wealth at any time are classified as high-capital investors, and all remaining investors are classified as general investors.

The high-capital and actively trading investor classifications measure two aspects of investing experience—the amount of wealth invested and the frequency of trades. We use these two aspects of investing experience to proxy for individual investor sophistication. With respect to the amount of wealth invested, an investor who has a greater amount of wealth invested has a greater incentive to learn about stock trading.[17] Furthermore, greater invested wealth may be associated with past stock market success. With respect to the frequency of trades, investors may learn through experience about the time-series properties of earnings and about market price patterns. This suggests that more sophisticated individual investors may be better at avoiding errors in trading in response to earnings announcements, or may even be good at exploiting PEAD.

Sample Selection and Variable Definitions

Our sample consists of all firm-quarters (with sufficient Compustat data) with at least one trade made by our sample of investors during the 25 days following an earnings announcement. From Compustat, we require primary earnings per share before extraordinary items (quarterly data item 19) at both quarter t and quarter t–4, price per share at the end of quarter t (item 14), and the corresponding split-adjustment factors (item 17). Additionally, we require that the earnings announcement date and the number of shares outstanding at the end of the quarter (item 61) be available. Using the Compustat data, we construct the standardized unexpected earnings (SUE) as the seasonal difference in split-adjusted earnings per share scaled by the split-adjusted end of quarter price (i.e., the price at the end of the quarter prior to the earnings announcement), similar to Bernard et al. (1997). We calculate SUE for all New York Stock Exchange (NYSE) firm-quarters with sufficient data available, and using this data, we define SUE 1 observations as the 10 percent of firm-quarters with the most negative random walk earnings surprise, SUE 10 observations as the 10 percent of firm-quarters with the most positive random walk earnings surprise, and SUE 5 and 6 observations as the 20 percent of firm-quarters with the smallest (in absolute value) random walk earnings surprise.

For each firm-quarter, we identify all trades of the firm's common stock made by our sample of investors during the following quarter. We measure their trading activity over various event windows, ranging in length from one day to a whole quarter. For example, we measure the trading activity on the earnings announcement day for quarter q, for firm j, by summing the number of common shares of firm j traded on the earnings announcement day by any investor in the data set. Following Burch and Swaminathan (2003), we scale by the number of common shares outstanding for firm j at the end of quarter q. We repeat this procedure for subsamples of trades (i.e., for buys and sells) and for subsamples of investors (i.e., for high-capital investors, actively trading investors, and general investors). We measure net purchases as the difference between the number of shares purchased and the number of shares sold in the event window, scaled by millions of shares outstanding at quarter-end.[18]

To address the possibility that individuals rationally trade differently from other investors in response to an earnings surprise, in our tests, we examine the deviation between individual investor trading of firms with extreme earnings surprises and individual investor trading of firms with little or no earnings surprise. That is, we use individual trades in the shares of firm-quarters in SUE 5 and 6 as the benchmark, and test how the trades of shares of firm-quarters in SUE 1 and of firm-quarters in SUE 10 differ from this benchmark.

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[17] Consistent with this, Cready (1988) finds that wealthy institutional investors trade more quickly in response to earnings announcements, suggesting that the value of information increases with wealth.

[18] When the number of shares purchased exceeds the number of shares sold in the event window, net purchases are positive. When the number of shares sold exceeds the number of shares purchased in the event window, net purchases are negative.

Descriptive Statistics

Our final sample consists of 539,239 trades made in the 25 days following 51,627 earnings announcements. 54 percent of these trades are buys, with a mean number of shares purchased of 530, and 46 percent of these trades are sells, with a mean number of shares sold of 623.[19] Although 76.9 percent of the investors are classified as general investors, these investors make only 46 percent of the trades, and the 15.4 percent of the sample that is classified as high-capital investors make only 13 percent of the trades. The remaining 41 percent of the trades are made by the 7.7 percent of the sample that is classified as actively trading investors. Further descriptive statistics are provided in Table 1.

Panel A of Table 1 describes the distribution of trade sizes for both buys and sells by year. It is interesting to note the large number of large trades in the database. For example, the trade size is greater than $5,000 for approximately half of the trades, and at least 500 shares are traded in more than 25 percent of the trades. Prior studies use either the dollar value of the transaction or the number of shares traded to classify trades as being initiated by individuals or institutions, and using their cutoffs, prior studies would incorrectly classify these large trades as either institutional trades or as indeterminate. The wide variation in the frequency of trading among individuals is also of interest.

Panel B of Table 1 shows that while the median individual trades four times per year for a total of approximately $21,000, the median actively trading investor trades 22 times a year for approximately $158,000. It is also interesting to note how skewed the trading volume (measured in dollar value and in number of trades) is. For example, the mean trading volume, measured in dollars per year, is greater than the third quintile, indicating that there are a few very large trades. Finally, actively trading investors trade, on average, more than six times as often and more than ten times as much (in dollar value) as general investors, and more than four times as often and more than five times as much (in dollar value) as high-capital investors. Because the actively trading investors are indeed highly active, these investors may be disproportionately important in generating empirically observed price patterns. On the other hand, these traders may be more sophisticated than other individual investors, suggesting that they are not the source of PEAD.

IV. INDIVIDUAL INVESTOR TRADING AS A PREDICTOR OF POST-EARNINGS ANNOUNCEMENT DRIFT

As discussed in Section II, if trading by individual investors causes PEAD, then their net trading should negatively predict subsequent stock market returns. Furthermore, net trading by individuals should subsume part or all of the explanatory power of the earnings surprise for predicting subsequent abnormal stock returns. In this section, we examine the relation between individual investor trading and subsequent market-adjusted returns for the sample of investors as a whole and for the individual investor classes described previously.

Trading by Individual Investors and Subsequent Returns

Previous studies (e.g., Bernard and Thomas 1989, 1990) find that PEAD is strongest among firms with relatively extreme earnings surprises, suggesting that tests focusing on firms with

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[19] Table 1 descriptive statistics differ slightly from these because the Table 1 sample includes all trades of common stock made by investors in the dataset and is not restricted to trades of firms with available Compustat data.

TABLE 1
Descriptive Statistics

Panel A: Distribution of Trade Size in Dollars (Shares)

		$ Trade Size				
Year	**Buy/Sell**	**Mean**	**Quintile 1**	**Median**	**Quintile 3**	**Trades**
1991	Buy	$9,654	$2,213	$4,425	$9,325	197,277
		(653)	(100)	(200)	(500)	
1991	Sell	$12,399	$2,500	$5,200	$11,563	144,156
		(720)	(100)	(250)	(700)	
1992	Buy	$10,244	$2,388	$4,688	$9,625	186,819
		(645)	(100)	(200)	(500)	
1992	Sell	$12,957	$2,550	$5,400	$12,000	141,101
		(753)	(100)	(255)	(800)	
1993	Buy	$10,381	$2,500	$4,838	$10,000	181,008
		(700)	(100)	(200)	(600)	
1993	Sell	$12,385	$2,625	$5,450	$12,050	155,292
		(710)	(100)	(250)	(700)	
1994	Buy	$10,542	$2,475	$4,863	$10,250	152,609
		(695)	(100)	(250)	(700)	
1994	Sell	$12,508	$2,539	$5,500	$12,375	128,072
		(713)	(100)	(300)	(702)	
1995	Buy	$12,942	$2,788	$5,625	$12,125	178,391
		(689)	(100)	(225)	(700)	
1995	Sell	$14,961	$2,800	$6,125	$14,325	160,005
		(716)	(100)	(296)	(800)	
1996	Buy	$13,495	$2,900	$5,700	$12,750	186,003
		(700)	(100)	(200)	(700)	
1996	Sell	$16,554	$3,113	$6,768	$16,050	158,968
		(727)	(100)	(300)	(800)	

Panel B: Distribution of Dollar Volume (Number) of Trades per Investor-Year by Investor Class

Investor Type	**Mean**	**Q1**	**Median**	**Q3**	**Number of Investor-Years**
All Investors	$125,503	$7,416	$21,256	$67,767	275,062
	(10.34)	(1)	(4)	(10)	
High-Capital Investors	$105,623	$13,984	$38,841	$101,975	52,482
	(8.60)	(2)	(5)	(11)	
Actively Trading Investors	$577,042	$51,039	$158,276	$449,038	33,068
	(38.65)	(8)	(22)	(46)	
General Investors	$52,220	$5,563	$14,428	$37,700	189,512
	(6.15)	(1)	(3)	(7)	

The sample consists of all trades of common shares made by investors from January 1991 to December 1996 inclusive.
In Panel A, the unit of measurement is a single trade by an investor.
In Panel B, the unit of measurement is a year of trading activity by a single investor.

extreme earnings surprises may be more powerful. Focusing on firms with extreme earnings surprises filters out the noise from firms with modest SUEs and little PEAD. Therefore, we restrict the sample for these analyses to those 4,405 firm-quarters in SUE deciles 1 and 10 with nonzero net buys in the five days following the earnings announcement.

Table 2 reports the results of regressions of abnormal returns (measured over the 3, 6, 9, and 12 months subsequent to the earnings announcement) on SUE (the decile rank of the earnings surprise), controlling for market-to-book ratio, size, and past momentum. To control for market-to-book, size, and momentum, we include as regressors in the first model the decile rank of the firm's market-to-book ratio (*MTB*), the decile rank of the firm's market value of equity (*MVE*), and the market-adjusted buy and hold returns over the six months prior to the earnings announcement date (momentum). The coefficients on SUE are highly significant in all returns windows, confirming that after controlling for size, market-to-book, and momentum, the PEAD effect was strong during our sample period. Thus, this time period is appropriate for testing whether individual investor trading drives PEAD.

We next examine the effect of including the decile rank of net purchases (*RANK NET PURCHASES*) in the second regression model. Specifically, *RANK NET PURCHASES* is the decile rank based on the number of shares purchased minus the number of shares sold in days +1 through +5 relative to the earnings announcement date, scaled by the number of shares outstanding at the end of the fiscal quarter for which earnings is announced. Table 2 reveals that individual investor trading in the five days following extreme earnings announcements (*RANK NET PURCHASES*) is a significant negative predictor of future 3-, 6-, 9-, and 12-month stock returns, and that this effect is independent of the size, market-to-book, and momentum effects.

Table 2 also reveals that the PEAD effect is *not* subsumed by individual investor trading. That is, in every window, the coefficient on SUE remains highly significant ($p < 0.0001$) when *RANK NET PURCHASES* is included in the regression. One could argue that only a subset of individual investors drive PEAD. If so, then including investors outside of this subset adds noise to our analyses and in this case, we may not expect *RANK NET PURCHASES* to subsume SUE completely. However, including *RANK NET PURCHASES* in the regression does not detract *at all* from the magnitude and significance of the SUE effect. Thus, the evidence strongly contradicts the individual trading hypothesis.

It is important to note that the failure of the individual trading hypothesis does not come from a lack of power. The test captures a strong and significant relation between SUE and future returns, and, after controlling for SUE, between *RANK NET PURCHASES* and future returns. Indeed, the coefficients on *RANK NET PURCHASES* from regressions that include SUE are larger and more significant than those from regressions (untabulated) that do not include SUE. Thus, individual investors have a special "skill" at picking losers, conditional on an extreme earnings surprise.[20]

In summary, these analyses reveal that individual investors trade foolishly in response to extreme earnings surprises in the sense that their trades in the five days following these surprises are negative predictors of returns over the next 3, 6, 9, and 12 months. This anti-arbitrage by individual investors suggests that they may be the driving force behind some kind of market inefficiency, perhaps losing money when the market misvaluation is corrected. However, this individual trading effect appears to be unrelated to PEAD. That is, there is no indication from the returns evidence that individual investors drive PEAD, nor is there any indication that PEAD underlies this individual investor trading effect.

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[20] This is consistent with evidence, not conditioned on earnings surprises, that individual investor trades on average underperform (Odean 1999; Barber and Odean 2000).

TABLE 2

Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (*MTB*), Size (*MVE*), *Momentum*, and *RANK NET PURCHASES* for All Investors

(n = 4,405)

Returns Window	Intercept	SUE	*MTB*	*MVE*	*Momentum*	*RANK NET PURCHASES*	Adj. R^2
3 Months	0.025	0.005	−0.003	−0.006	0.009		1.17%
	(2.18)	(5.31)	(−1.69)	(−4.27)	(1.08)		
	0.040	0.006	−0.003	−0.006	0.008	−0.003	1.30%
	(3.14)	(5.41)	(−1.69)	(−4.41)	(0.87)	(−2.61)	
6 Months	0.039	0.009	−0.006	−0.009	0.027		1.38%
	(2.22)	(5.40)	(−2.68)	(−3.98)	(2.02)		
	0.064	0.009	−0.006	−0.009	0.024	−0.005	1.52%
	(3.21)	(5.50)	(−2.67)	(−4.14)	(1.08)	(−2.67)	
9 Months	0.045	0.010	−0.011	−0.008	0.051		1.61%
	(2.14)	(5.34)	(−4.06)	(−2.95)	(3.22)		
	0.068	0.010	−0.011	−0.008	0.048	−0.005	1.68%
	(2.86)	(5.42)	(−4.06)	(−3.07)	(3.04)	(−2.08)	
12 Months	0.096	0.013	−0.017	−0.011	0.080		1.68%
	(3.36)	(4.89)	(−4.41)	(−3.03)	(3.70)		
	0.131	0.013	−0.017	−0.011	0.076	−0.007	1.77%
	(4.04)	(4.98)	(−4.41)	(−3.16)	(3.51)	(−2.30)	

Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3, 6, 9, or 12 months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period.

SUE includes those 4,405 firm-quarters in SUE deciles 1 and 10 with nonzero net buys in the five days following the earnings announcement.

MTB = decile rank of the firm's market-to-book ratio;
MVE = decile rank of the firm's market value of equity;
Momentum = market-adjusted buy and hold returns for the six months prior to the earnings announcement date; and
RANK NET PURCHASES = calculated as the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Trading by Classes of Individual Investors and Future Returns

The results in the previous section rule out the hypothesis that the trades made by the individuals in our sample *in aggregate* are the source of drift. However, it remains possible that some unsophisticated class of individuals drives PEAD, and that this is being masked by sophisticated trades in the opposite direction made by another subset of individuals. Thus, to explore further whether a class of individuals drives PEAD, we report similar returns regressions in Table 3 for each of the investor classes. Most regressors other than *RANK NET PURCHASES* remain significant and for all investor classes, the coefficients on *RANK NET PURCHASES* remain negative, but are insignificant in some cases.

The significance and magnitude of the coefficients on *RANK NET PURCHASES* increase from actively trading investors to high-capital investors to general investors. The results suggest that actively trading investors are less "skilled" at picking losers following extreme earnings surprises, that ranked net purchases made by high-capital investors following extreme earnings surprises are stronger predictors of negative future returns, and that this effect is strongest for general investors. The key finding, however, is that for all classes of individual investors, adding *RANK NET PURCHASES* to the regression has virtually no effect on the coefficient estimate or significance of SUE. That is, SUE is not subsumed by the trading of any class of investors. This evidence strongly opposes the hypothesis that trading by any of these investor classes drives PEAD.

Trading by Individual Investors in Different Stock Categories and Subsequent Returns

As a further robustness check, we reconsider whether trading by individual investors can explain PEAD better in subsamples of stocks that are likely to be less efficiently priced, and whose prices are more likely to be influenced by individual investor trading. We therefore perform return prediction tests in subsamples of firms with no analyst following, with low stock prices, or with low market value of equity (i.e., small firms). Past research indicates that such firms have a poorer information environment, and, owing to higher transactions costs, are more difficult to arbitrage.[21]

Table 4 presents our findings on the prediction of three-month-ahead returns. The main finding using these subsamples is the same as that in the full-sample tests. The first set of regressions considers firms with no analyst following. In a regression of returns on SUE, market-to-book, market value of equity, and momentum, we find that adding the variable *RANK NET PURCHASES* has essentially no effect on the coefficient on SUE. (The coefficient is 0.007 in both regressions and the t-statistic is 3.58 without *RANK NET PURCHASES* and is virtually identical at 3.65 when *RANK NET PURCHASES* is included in the model.) In other words, considering only those firms not followed by analysts, the trades of individual investors do not help to explain drift. Although not relevant for our main conclusion, we note that in contrast to the full sample findings, the coefficient on *RANK NET PURCHASES* is no longer significant. This may be due to lower power since there are fewer observations in this subsample, and firms with no analyst following are likely to have greater return volatility.

The second set of regressions considers firms with low stock prices. Adding the variable *RANK NET PURCHASES* has little effect on the coefficient on SUE, which increases slightly from 0.006 to 0.007. Once again, the trades of individual investors do not subsume the predictive power

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[21] Mashruwala et al. (2006) find that the accrual anomaly is found in low-price and low-volume stocks and suggest that transaction costs can impose a barrier that prevents investors from arbitraging mispricing. Similarly, Ng et al. (2008) find that PEAD is stronger for firms with higher transaction costs. Prior literature also finds that PEAD is more prevalent in small firms (Foster et al. 1984; Bernard and Thomas 1989, 1990) and since individual investors tend to be disproportionate holders of shares of small firms, several authors suggest that smaller firms are more likely to have a less sophisticated shareholder base (Lee et al. 1991; Potter 1992; Walther 1997).

TABLE 3

Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (*MTB*), Size (*MVE*), *Momentum*, and *RANK NET PURCHASES* by Investor Classes

Panel A: High-Capital Investors (n = 1,914)

Returns Window	Intercept	SUE	*MTB*	*MVE*	*Momentum*	*RANK NET PURCHASES*	Adj. R^2
3 Months	0.121	0.001	−0.005	−0.011	0.011		1.68%
	(6.31)	(0.98)	(−2.21)	(−4.77)	(0.82)		
	0.135	0.001	−0.005	−0.011	0.010	−0.003	1.74%
	(6.32)	(0.97)	(−2.19)	(−4.82)	(0.73)	(−1.46)	
6 Months	0.148	0.003	−0.009	−0.014	0.023		1.37%
	(4.96)	(1.41)	(−2.56)	(−3.83)	(1.08)		
	0.163	0.003	−0.009	−0.014	0.021	−0.003	1.38%
	(4.93)	(1.40)	(−2.55)	(−3.87)	(1.01)	(−1.05)	
9 Months	0.200	0.003	−0.015	−0.015	0.052		1.83%
	(5.59)	(1.03)	(−3.62)	(−3.61)	(2.06)		
	0.215	0.003	−0.015	−0.016	0.051	−0.003	1.81%
	(5.40)	(1.02)	(−3.61)	(−3.64)	(2.01)	(−0.84)	
12 Months	0.317	0.003	−0.013	−0.020	0.114		2.70%
	(6.50)	(0.82)	(−4.90)	(−3.44)	(3.30)		
	0.350	0.003	−0.028	−0.020	0.111	−0.007	2.75%
	(6.45)	(0.81)	(−4.88)	(−3.49)	(3.21)	(−1.37)	

(continued on next page)

TABLE 3 (continued)

Panel B: Actively Trading Investors (n = 3,261)

Returns Window	Intercept	SUE	*MTB*	*MVE*	*Momentum*	*RANK NET PURCHASES*	Adj. R^2
3 Months	0.039	0.006	−0.003	−0.007	0.029		1.87%
	(2.82)	(4.68)	(−1.87)	(−4.47)	(2.88)		
	0.040	0.006	−0.003	−0.007	0.029	−0.000	1.84%
	(2.60)	(4.68)	(−1.87)	(−4.47)	(2.88)	(−0.07)	
6 Months	0.037	0.011	−0.008	−0.009	0.056		2.46%
	(1.74)	(5.89)	(−2.99)	(−3.52)	(3.67)		
	0.045	0.011	−0.008	−0.009	0.055	−0.002	2.46%
	(1.97)	(5.93)	(−3.00)	(−3.53)	(3.64)	(−0.93)	
9 Months	0.039	0.012	−0.013	−0.008	0.078		2.78%
	(1.60)	(5.97)	(−4.14)	(−2.70)	(4.42)		
	0.052	0.013	−0.013	−0.008	0.078	−0.003	2.79%
	(1.93)	(6.02)	(−4.14)	(−2.71)	(4.38)	(−1.16)	
12 Months	0.116	0.014	−0.020	−0.011	0.114		2.49%
	(3.34)	(4.72)	(−4.76)	(−2.67)	(4.55)		
	0.135	0.014	−0.021	−0.011	0.112	−0.005	2.51%
	(3.57)	(4.77)	(−4.77)	(−2.69)	(4.51)	(−1.27)	

(continued on next page)

TABLE 3 (continued)

Panel C: General Investors (n = 3,293)

Returns Window	**Intercept**	**SUE**	***MTB***	***MVE***	***Momentum***	***RANK NET PURCHASES***	**Adj. R^2**
3 Months	0.038	0.006	−0.003	−0.008	−0.002		1.34%
	(2.79)	(4.92)	(−1.48)	(−4.57)	(−0.21)		
	0.060	0.006	−0.003	−0.008	−0.004	−0.004	1.56%
	(3.84)	(5.01)	(−1.43)	(−4.81)	(−0.38)	(−2.89)	
6 Months	0.050	0.010	−0.008	−0.010	0.022		1.65%
	(2.40)	(5.23)	(−2.83)	(−3.74)	(1.48)		
	0.079	0.010	−0.008	−0.010	0.020	−0.006	1.81%
	(3.32)	(5.31)	(−2.79)	(−3.95)	(1.34)	(−2.52)	
9 Months	0.056	0.010	−0.013	−0.007	0.056		1.93%
	(2.32)	(4.89)	(−4.21)	(−2.46)	(3.23)		
	0.087	0.011	−0.013	−0.008	0.053	−0.006	2.06%
	(3.15)	(4.96)	(−4.17)	(−2.66)	(3.09)	(−2.31)	
12 Months	0.120	0.013	−0.020	−0.011	0.077		1.97%
	(3.63)	(4.49)	(−4.59)	(−2.76)	(3.23)		
	0.162	0.013	−0.019	−0.012	0.073	−0.009	2.10%
	(4.29)	(4.56)	(−4.55)	(−2.95)	(3.09)	(−2.30)	

Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3, 6, 9, or 12 months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period.

SUE includes those 4,405 firm-quarters in SUE deciles 1 and 10 with nonzero net buys in the five days following the earnings announcement.

MTB = decile rank of the firm's market-to-book ratio;
MVE = decile rank of the firm's market value of equity;
Momentum = market-adjusted buy and hold returns for the six months prior to the earnings announcement date; and
RANK NET PURCHASES = calculated as the decile rank of the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 4

For High Transaction Cost/Low Information Firms
Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (*MTB*), Size (*MVE*), *Momentum*, and *RANK NET PURCHASES* for All Investors

3 Month Returns Window	Intercept	SUE	*MTB*	*MVE*	*Momentum*	*RANK NET PURCHASES*	Adj. R^2
Firms with No Analyst Following	0.031	0.007	−0.004	−0.010	−0.005		1.11%
n = 1,763	(1.54)	(3.58)	(−1.51)	(−2.92)	(−0.34)		
	0.046	0.007	−0.004	−0.010	−0.005	−0.003	1.16%
	(1.98)	(3.65)	(−1.48)	(−2.94)	(−0.41)	(−1.32)	
Low Stock Price Firms	0.017	0.006	−0.003	−0.005	−0.015		0.63%
n = 2,094	(0.91)	(3.66)	(−1.08)	(−1.47)	(−1.10)		
	0.043	0.007	−0.003	−0.005	−0.017	−0.006	0.88%
	(1.99)	(3.77)	(−1.10)	(−1.45)	(−1.28)	(−2.49)	
Small Firms	0.016	0.007	−0.001	−0.009	0.004		0.90%
n = 2,311	(0.85)	(4.15)	(−0.40)	(−2.31)	(0.35)		
	0.036	0.007	−0.001	−0.009	0.003	−0.004	1.03%
	(1.71)	(4.22)	(−0.39)	(−2.30)	(0.23)	(−2.05)	

Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3 months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period.

SUE includes those firm-quarters in SUE deciles 1 and 10 with nonzero net buys in the five days following the earnings announcement and no analysts' forecasts of earnings in the month prior to the earnings announcement.

MTB = decile rank of the firm's market-to-book ratio;
MVE = decile rank of the firm's market value of equity;
Momentum = market-adjusted buy and hold returns for the six months prior to the earnings announcement date; and
RANK NET PURCHASES = calculated as the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

of SUE at all. In this subsample, as in the full sample, the coefficient on *RANK NET PURCHASES* is negative and significant, which indicates that individual investors lose money on their trades of firms with low stock prices.

The third set of regressions considers small firms. Here, adding the variable *RANK NET PURCHASES* has essentially no effect on the coefficient on SUE, so in this subsample as well, the trades of individual investors do not help to explain drift. As in the full sample, the coefficient on *RANK NET PURCHASES* is negative and significant, indicating that individual investors lose money on their trades of small firms.

We also perform similar tests (untabulated) for the prediction of returns six and nine months ahead. The findings are similar. In any of our subsamples or returns prediction horizons, individual trading does not subsume the ability of SUE to predict returns.

V. INDIVIDUAL INVESTOR TRADING FOLLOWING EXTREME EARNINGS SURPRISES

Trading by Individual Investors following Extreme Earnings Surprises

The individual trading hypothesis implies that individuals will buy after extremely bad earnings news (pushing the stock price up) and sell after extremely good earnings news (pushing the stock price down). As described previously, in the first set of tests that follow, we examine the trades made by individuals following extreme earnings surprises (SUE 1 firm-quarters and SUE 10 firm-quarters) and compare these to the trades made by individual investors following earnings announcements with little or no surprise (SUE 5 and 6).

Figure 1 reveals that in the 25 trading days following an extreme earnings surprise, cumulative abnormal net purchases made by individuals are greater on average for SUE 1 (bad news) firm-quarters than for SUE 10 (good news) firm-quarters.[22] However, two aspects of this evidence sharply contradict the proposition that individual investors cause PEAD. First, individuals are net purchasers after both good news and bad news. This confirms the findings in Lee (1992) for extreme earnings news, and suggests that the earnings attention effect documented by Lee (1992) is due, at least in part, to individual investors. The net buying by individuals in SUE 10 firm-quarters during the 16 days following an extreme earnings surprise is inconsistent with the hypothesis that individual investors trade against favorable earnings news, causing under-reaction and subsequent drift. Second, the difference in cumulative net purchases between good news and bad news firm-quarters does not develop until 17 days (i.e., more than three weeks) after the earnings announcement, so differences in individual trading do not seem to explain any under-reaction or over-reaction in the days following the earnings announcement.

Table 5 reports abnormal trading behavior following the initial earnings announcement and provides statistics that confirm the pattern in Figure 1. Panel A (B) reports slope coefficients and t-statistics from separate regressions of buys, sells, and net purchases per million shares outstanding on an indicator variable set equal to 1 for extreme good news (SUE 10) (extreme bad news (SUE 1)) firms, and 0 for firms in SUE 5 and SUE 6 (i.e., for those with little or no earnings surprise).

In the first 25 days following an extreme earnings surprise, there is statistically significant buying and selling in both good and bad news firm-quarters (relative to no news firm-quarters). However, the difference (untabulated) between the net purchases following good versus bad news

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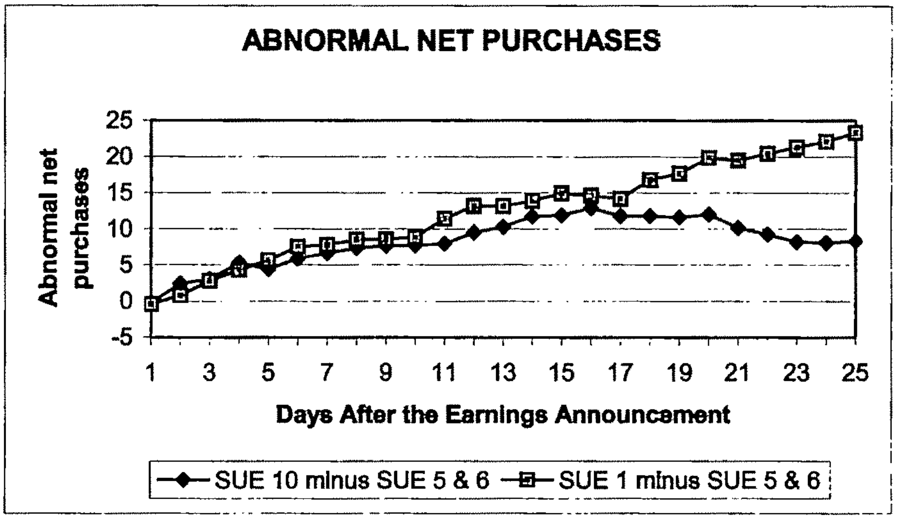
[22] Cumulative net purchases is the sum of shares purchased minus the sum of shares sold beginning on the day following the earnings announcement and ending on day *t*, scaled by the number of shares outstanding at the end of the quarter for which earnings is announced. Cumulative abnormal net purchases is the difference between cumulative net purchases for SUE 1 (SUE 10) firm-quarters and cumulative net purchases for SUE 5 and 6 firm-quarters.

FIGURE 1
Cumulative Abnormal Net Purchases Relative to the Earnings Announcement Date[a,b]



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[a] SUE 1 observations are firm-quarters with the most negative random walk earnings surprise, SUE 10 observations are firm-quarters with the most positive random walk earnings surprise, and SUE 5 and 6 observations are firm-quarters with the smallest random walk earnings surprise (in absolute value).

[b] Net purchases is measured as the number of shares purchased minus the number of shares sold, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

is not significant in the first three weeks of trading following an earnings announcement. Overall, this evidence suggests that individuals are influenced by an earnings attention effect, but there is no indication that individuals systematically engage in the earnings-contrarian form of trading that would induce under-reaction and cause PEAD.

Trading by Individual Investor Class

Next, we examine the trading behavior of those individual investor classes that are most likely to be either less or more sophisticated. As discussed previously, we posit that investors with more capital invested and investors who trade more actively may be more sophisticated in their processing of information. We therefore study trading by investor class to test whether general investors drive PEAD, and whether high-capital investors and actively trading investors arbitrage PEAD.

Panel A (Panel B) of Table 6 tests whether net purchases are significantly different following extreme good news (bad news) earnings announcements measured relative to the non-news case for each of the three classes of investors (high-capital investors, actively trading investors, and general investors).

Comparing Panels A and B of Table 6, we find that general investors are net purchasers after bad earnings news, and rather weakly after good earnings news, which is fairly consistent with an earnings attention effect. Contrary to the individual trading hypothesis, there is no sign that the

TABLE 5

Abnormal Trading following Extreme Earnings Surprises Regressions on SUE Indicators

Panel A: Good News Firm-Quarters (SUE 10) versus No News Firm-Quarters (SUE 5 and 6)

n = 16,545 (firm-quarters)	Buys	Sells	Net Purchases
Days +1 through +5	47.332	41.492	5.841
	(<0.0001)	(<0.0001)	(0.158)
Days +6 through +15	56.346	44.633	11.713
	(<0.0001)	(<0.0001)	(0.002)
Days +16 through +25	52.045	50.753	1.292
	(<0.0001)	(<0.0001)	(0.756)

Panel B: Bad News Firm-Quarters (SUE 1) versus No News Firm-Quarters (SUE 5 and 6)

n = 11,888 (firm-quarters)	Buys	Sells	Net Purchases
Days +1 through +5	46.232	26.862	19.370
	(<0.0001)	(<0.0001)	(<0.0001)
Days +6 through +15	73.644	65.548	8.095
	(<0.0001)	(<0.0001)	(0.162)
Days +16 through +25	87.024	57.301	29.723
	(<0.0001)	(<0.0001)	(<0.0001)

Cells in Panel A (B) contain coefficients and p-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 (SUE 1), and set equal to 0 for firm-quarters in SUE 5 and 6.

The columns measure the Buys, Sells, and Net Purchases after an earnings announcement (quarter 0).

Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

general investors sell after good news. Furthermore, we find no evidence that more actively trading or high-capital individual investors exploit PEAD. (To do so, they would have to sell after good news and buy after bad news.) In fact, for high-capital investors, net purchases after good news are insignificant, and after bad news, are significantly *positive* during days +1 through +15. While trading by actively trading investors is consistent with an earning attention effect (i.e., net buying in some windows after both good and bad news), since these investors are not net sellers after bad news, they do not seem to be taking advantage of PEAD.

Trading by Individual Investors in Different Stock Categories

As a further robustness check, we again examine individual trading in response to earnings announcements in the subsamples of stocks that are more likely to be inefficiently priced and to be influenced by individual investor trading. We therefore perform the trading tests in subsamples of firms with no analyst following, low stock prices, or low market value of equity (i.e., small firms).

TABLE 6

Net Purchases by Individual Investor Classes at the Initial Earnings Announcement

Panel A: Good News Firm-Quarters (SUE 10) versus No News Firm-Quarters (SUE 5 and 6)

	High-Capital n = 5,334	Actively Trading n = 11,797	General n = 12,180
Net purchases days +1 through +5	3.539	1.700	4.550
	(0.167)	(0.696)	(0.168)
Net purchases days +6 through +15	−0.360	12.085	4.387
	(0.863)	(0.003)	(0.084)
Net purchases days +16 through +25	−0.743	5.711	−3.542
	(0.761)	(0.223)	(0.196)

Panel B: Bad News Firm-Quarters (SUE 1) versus No News Firm-Quarters (SUE 5 and 6)

	High-capital n = 6,993	Actively Trading n = 8,541	General n = 8,826
Net purchases days +1 through +5	14.178	2.766	17.514
	(0.000)	(0.488)	(0.000)
Net purchases days +6 through +15	15.811	4.701	−1.479
	(0.000)	(0.323)	(0.796)
Net purchases days +16 through +25	3.652	22.033	18.854
	(0.319)	(0.001)	(0.000)

Cells in Panel A (B) contain coefficients and p-values, below in parentheses, from a regression of net purchases on an indicator variable equal to 1 for firm-quarters in SUE 10 (SUE 1), and set equal to 0 for firm-quarters in SUE 5 and 6, and a measure of market-wide trading.

Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Table 7, Panel A considers trading in days +1 through +5 after good news announcements, and Panel B considers trading in response to bad news announcements. As before, we report trading relative to the amount of trading in SUE 5 and 6 (no news), so this is a measure of the abnormal trading associated with extreme good or bad news. The panels provide results for firms with no analyst following, for low price firms, and for small firms.

For each of these categories, net purchases are significant after bad news. This is potentially consistent with trading by individual investors hindering downward price adjustment. However, after good news there is no sign of net sales; the point estimates for all three categories of firms indicate positive net purchases, though the coefficients are insignificant. Thus, there is no sign of investor selling after good news and so no evidence suggesting that individual trading hinders upward price adjustment after good news.

However, two aspects of this evidence oppose the proposition that individual investors cause PEAD. First, individuals are net purchasers after both good news and bad news. This confirms the findings in Lee (1992) for extreme earnings news, and suggests that the earnings attention effect documented by Lee (1992) is due, at least in part, to individual investors. Second, the difference in cumulative net purchases between good news and bad news firm-quarters does not develop until

TABLE 7

For High Transaction Cost/Low Information Firms Abnormal Trading following Extreme Earnings Surprises: Regressions on SUE Indicators

Panel A: Good News Firm-Quarters (SUE 10) versus No News Firm-Quarters (SUE 5 and 6)[a]

Trading Days +1 through +5	Buys	Sells	Net Purchases
Firms With No Analyst Following	57.698	48.790	8.709
	(<0.0001)	(<0.0001)	(0.507)
Low Stock Price Firms	45.798	39.035	6.677
	(<0.0001)	(<0.0001)	(0.458)
Small Firms	49.757	37.700	12.016
	(<0.0001)	(<0.0001)	(0.215)

Panel B: Bad News Firm-Quarters (SUE 1) versus No News Firm-Quarters (SUE 5 and 6)[b]

Trading Days +1 through +5	Buys	Sells	Net Purchases
No Analyst Following	42.179	19.284	23.351
	(<0.0001)	(<0.0020)	(<0.0250)
Low Stock Price Firms	37.402	14.731	24.401
	(<0.0001)	(<0.0001)	(<0.0001)
Small Firms	38.818	12.562	28.002
	(<0.0001)	(0.0290)	(<0.0001)

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[a] The sample sizes are as follow: for Firms with No Analysts Following, 4,823 observations; for Low Price Firms, 7,767 observations; for Small Firms, 7,190 observations.

[b] The sample sizes are as follow: for Firms with No Analyst Following, 2,558 observations; for Low Stock Price Firms, 4,704 observations; for Small Firms, 4,289 observations.

Cells in Panel A (B) contain coefficients and p-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 (SUE 1), and set equal to 0 for firm-quarters in SUE 5 and 6.

The columns measure the Buys, Sells, and Net Purchases after an earnings announcement (quarter 0).

Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

17 days (i.e., more than three weeks) after the earnings announcement, so differences in individual trading cannot explain any under-reaction or over-reaction in the days following the earnings announcement.

In summary, the evidence on individual investor trading does not support the hypothesis that individuals or any class thereof are systematically trading in a manner that would cause PEAD. Nor does it support the hypothesis that any of the classes of individuals that we examine is systematically trading in order to exploit PEAD.

VI. TRADING IN SHORT WINDOWS AROUND THE SUBSEQUENT EARNINGS ANNOUNCEMENT

The individual trading hypothesis predicts that individual investors are net sellers after initial positive earnings surprises, and more strongly, are net purchasers after initial negative earnings surprises. As discussed in Section II, if drift represents genuine mispricing, then sophisticated investors, who understand that the drift is particularly intense near the dates of subsequent earnings announcements, should also time their trades with respect to the *subsequent* announcements. Specifically, after a positive earnings surprise, they should avoid selling immediately before the next quarterly earnings announcement, and instead delay selling until after the announcement. Furthermore, sophisticated investors should accelerate any planned purchases so that they are made immediately before the next quarterly earnings announcement. If sophisticated arbitrageurs follow this strategy, then for markets to clear, unsophisticated investors who are driving the mispricing must display an opposite trading pattern. Thus, as a final test of whether individuals are driving PEAD, we also examine investor trades in the days surrounding the subsequent earnings announcement, conditional on an initial extreme earnings surprise. Because PEAD is strongest at the first subsequent earnings announcement following a large surprise, we tabulate results relative to this earnings announcement (i.e., to quarter +1). However, the results (untabulated) in quarters +2 through +4 are consistent with quarter +1 results.

In Table 8, we report the Buys, Sells, and Net Purchases in four windows around the first earnings announcement following an extreme earnings surprise. Panel A (B) reports slope coefficients and p-values from separate regressions of Buys, Sells, and Net Purchases per million shares outstanding on an indicator variable set equal to 1 for SUE 10 (SUE 1) firm-quarters, and 0 for SUE 5 and 6 firm-quarters (i.e., for those firm-quarters with little or no earnings surprise).

Panel A of Table 8 shows that after an extreme positive earnings surprise, the number of shares both purchased and sold is unusually high and strongly significant in the 10 days preceding and in the 25 days following the quarter +1 earnings announcement, and Panel B shows that a similar pattern obtains after an extreme negative surprise. Thus, extreme earnings surprises trigger trading activity not only near the announcement, but in the days surrounding the subsequent quarterly earnings announcements. This is consistent with rational information-based trading or with an attention effect over a long horizon.

Turning to net purchases, conditional on good earnings news (see Panel A of Table 8), net buying is significantly positive on days −1 to −10 relative to the earnings announcement in quarter +1. This is not consistent with the hypothesis that individuals are naïvely driving the concentration of upside drift at later earnings announcement dates. Rather, the evidence is consistent with individual investors being sophisticated enough to accelerate buying to immediately before, rather than immediately after, the earnings announcement.

In summary, this evidence does not suggest that individuals are systematically trading in a way that would cause a concentration of drift around later quarterly earnings announcement dates. Nor does the evidence support the opposite hypothesis—that individual investors profit by systematically trading in a sophisticated fashion (i.e., by exploiting the drift at later earnings announcement dates).

Panel B of Table 8 is especially relevant for the individual trading hypothesis since drift is stronger after bad news than after good news. This panel reveals significant net buying in the five days subsequent to the quarter +1 earnings announcement following bad news, and some indication of further buying in days +16 through +25. This suggests sophisticated behavior because by delaying net purchases to a few days after the announcement, individuals are able to avoid the concentration of downward drift on the quarter +1 earnings announcement date.

TABLE 8

Abnormal Trading around the First Earnings Announcement following Extreme Earnings Surprises

Panel A: Good News Firm-Quarters (SUE 10) versus No News Firm-Quarters (SUE 5 and 6)

n = 16,545 (firm-quarters)	**Buys**	**Sells**	**Net Purchases**
Days −1 through −10	43.977	38.119	5.859
	(<0.0001)	(<0.0001)	(0.090)
Days +1 through +5	39.578	41.286	−1.708
	(<0.0001)	(<0.0001)	(0.640)
Days +6 through +15	56.121	50.196	5.925
	(<0.0001)	(<0.0001)	(0.140)
Days +16 through +25	48.367	55.223	−6.855
	(<0.0001)	(<0.0001)	(0.116)

Panel B: Bad News Firm-Quarters (SUE 1) versus No News Firm-Quarters (SUE 5 and 6)

n = 11,888 (firm-quarters)	**Buys**	**Sells**	**Net Purchases**
Days −1 through −10	46.935	51.961	−5.026
	(<0.0001)	(<0.0001)	(0.246)
Days +1 through +5	41.447	34.146	7.302
	(<0.0001)	(<0.0001)	(0.089)
Days +6 through +15	62.826	56.370	6.455
	(<0.0001)	(<0.0001)	(0.305)
Days +16 through +25	70.449	50.808	19.641
	(<0.0001)	(<0.0001)	(0.003)

Cells in Panel A (B) contain coefficients and t-statistics, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 (SUE 1), and set equal to 0 for firm-quarters in SUE 5 and 6.

Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

Taking the evidence as a whole (conditioned on either good or bad news), there is no indication that individuals are systematically engaging in a form of trading that would be expected if individual investor errors were the source of the concentration of drift at later quarterly earnings announcement dates. If anything, there is a rather modest indication that individuals are acting as sophisticated arbitrageurs to exploit PEAD.[23,24]

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[23] The evidence after bad news is, however, broadly suggestive of some psychological stories. After initial bad news about earnings, individuals are net buyers. This net buying could reflect an attention effect coupled with a disposition effect, or a bias in self-attribution (i.e., an insistence on interpreting new information as supportive of the self and past judgments (see, for example, Langer and Roth 1975)).

VII. CONCLUSION

This paper examines whether trading by individual investors drives post-earnings announcement drift, a proposition we call the individual trading hypothesis. At a descriptive level, several regularities are of interest. First, we document an earnings attention effect—extreme surprises trigger greater trading and greater net buying.[25] Second, the ability of individual trades to predict future returns (Odean 1999) extends to trades taken in response to extreme earnings surprises, and this effect is distinct from post-earnings announcement drift (PEAD). Third, the amount of abnormal trading is greater after extreme negative earnings surprises than after extreme positive earnings surprises, suggesting that bad news is highly salient.

Turning to the main question of the paper, we find no evidence that trading by individual investors following extreme earnings surprises causes PEAD. Such trading would need to impede the efficient adjustment of market prices to earnings surprises. In other words, if individuals were causing PEAD, then they would engage in earnings-contrarian trading—buying aggressively after extreme negative earnings news and selling after extreme positive earnings news. However, individuals are strongly significant net buyers in the first three weeks following both extreme positive and negative earnings surprises.

More importantly, we find direct evidence that, in our sample, individual investors in aggregate are *not* the source of PEAD. If trading by individuals was the source of PEAD, then their net purchases following an initial earnings announcement would subsume part or all of the ability of the earnings surprise to predict subsequent abnormal returns, but this is not the case. Although net buying by individuals in the five days following an extreme earnings surprise is a significant negative predictor of future abnormal returns, including ranked net purchases in a regression of abnormal returns on SUE does not at all weaken the ability of an extreme earnings surprise to predict returns. Nor does including the earnings surprise weaken the predictive power of individual trades. Thus, two distinct market inefficiencies seem to exist. The first is PEAD. The second is that the ranked net purchases made by individual investors in reaction to extreme earnings surprises are negative predictors of future abnormal returns. Further analysis by investor class suggests that none of our subcategories of investors are driving PEAD.

Finally, because PEAD is especially strong at the next quarterly earnings announcement following extreme negative earnings surprises, smart arbitrageurs should time their purchases to be immediately after (rather than immediately before) subsequent earnings announcements, and time their selling to be immediately before (rather than immediately after) subsequent earnings announcements. The reverse pattern is predicted after positive earnings surprises. If individual investors are naïve with respect to the concentration of PEAD at subsequent earnings announcements and are thereby impeding price adjustment, then we expect them to be making opposing trades. Thus, a further prediction of the individual trading hypothesis is that given a very negative earnings surprise, individuals will be net buyers immediately preceding the next earnings announcement, and will be net sellers immediately following the next earnings announcement. However, such patterns do not exist either for individuals in aggregate, or for investor classes based upon amount of capital invested or frequency of trading.

There are some limitations to the tests we perform. Although the number of observations in the sample is large, it includes only a random sample of individuals with accounts at a single

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[24] We also examined the trading behavior of the three categories of traders (high-capital, actively trading, and general) around earnings announcements following extreme earnings surprises and find results consistent with those for investors as a whole.

[25] Gervais et al. (2001) identify what they call the high-volume return premium, in which stocks with high volumes over the short term subsequently earn abnormally high average returns. Our findings suggest that individual investor buying after extreme earnings surprises may help to explain this effect.

major brokerage over a six-year period. Groups of individuals who do not use this brokerage may behave differently, and there may be further subcategories of individuals who cannot be identified using the information in the data set, who may behave differently.

Furthermore, as mentioned in footnote 14, a premise of our basic trading tests is that if individual investors drive drift, then their net purchases are positively correlated with the pressure they exert toward mispricing.[26] This assumption is intuitive. It will follow from the use as a benchmark of any rational setting in which different groups of investors behave sufficiently similarly. For example, in the capital asset pricing model (CAPM), all investors hold the market portfolio and a single earnings announcement has a negligible effect on the incentives for different investors to shift their holdings between the market portfolio and the risk-free asset. Thus, all investors have zero net purchases after earnings surprises. Relative to this benchmark, any observed net purchases create a pressure toward overpricing, and net sales create a pressure toward underpricing.

More generally, there are possible rational settings in which investors are more heterogeneous, so that earnings announcements trigger trading. If trading by an investor group is sufficiently naïve, so that members' trades systematically fluctuate widely from the trading implied by some rational benchmark model, then our premise that the net purchases of the group are positively correlated with pressure toward mispricing will still obtain.[27] However, a qualification to our conclusion is that there could be rational settings in which the premise of our basic trading tests are violated (i.e., where net buying is correlated with pressure toward underpricing). This situation would call into question the conclusions drawn from these tests.

In contrast, our return prediction tests do not rely heavily on the choice of a rational trading benchmark. Thus, if actual trades are informative about the pressure toward mispricing exerted by the investor group, then in a regression of post-earnings abnormal returns on investor trades and on the earnings surprise, investor trades should subsume part of the drift.[28] The fact that individual investor trades do not subsume any of the drift opposes the hypothesis that individuals drive drift.

In summary, we find no indication that trading by individual investors drives post-earnings announcement drift. As the discussion above indicates, there are limitations to our methodology. Given the surprising nature of our findings, future research using different data sets or methods to explore this issue further is warranted.

Our finding that individual investors do not seem to drive PEAD raises an obvious question: If individual investors do not cause PEAD, then who does? One possibility, as argued by some authors, is that PEAD is an artifact of poor risk-adjustment of returns, but this is not the predominant viewpoint in the literature at this time. A second possibility is that individuals drive PEAD in a way that cannot be identified using our data set. For example, some subcategory of individuals whose membership is unrelated to capital invested or trading experience may be naïve with respect to earnings announcements.

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[26] This assumption is implicit in other tests of trading behavior and drift. For example, Bhattacharya (2001) and Battalio and Mendenhall (2005) perform tests on the relation between earnings announcements and trades or trading volume measured relative to normal trading in a non-event window, rather than measured by adjusting trades for some rational benchmark (conditional on the value of the earnings surprise).

[27] Ideally, it would be desirable to calibrate a model of securities trading to provide a benchmark for rational individual investor trading after an earnings announcement. However, equilibrium in rational expectations models depends upon unobserved parameters, so it is not easy to calibrate these models to actual data.

[28] By coincidence, the irrational component of investor trades could exactly offset the rational component of the trades, so that individual investors drive drift, yet the net purchases of individuals always net to zero. However, this possibility is non-generic. Setting aside this set-of-measure-zero possibility, the trades that drive drift should be correlated (either positively or negatively) with the resulting drift.

Finally, it is possible that some subset of institutional investors generates PEAD, as a result of agency incentives or cognitive biases. For example, Frazzini (2006) provides evidence suggesting that mutual funds that have lost money on a stock are subject to the disposition effect, and that this bias on the part of fund managers can explain PEAD. Frazzini's (2006) findings and ours can be viewed as providing independent reinforcement for each other using very different methods and databases.

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Potential Functional and Dysfunctional Effects of Continuous Monitoring

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ABSTRACT: The trend toward continuous monitoring of automated business transactions by the internal audit function is growing as organizations seek to improve internal control. In this study, we demonstrate that continuous monitoring and the time horizon over which performance-contingent incentives are based can interact, thereby yielding potential functional and dysfunctional effects on managerial decisions. Seventy-two experienced corporate managers completed a between-participants experiment that randomized monitoring frequency (periodic or continuous) and incentive horizon (short-term or long-term). We found that earnings management of real activities significantly decreased as the frequency of monitoring increased in the presence of a short-term incentive horizon—a functional effect. However, with a long-term incentive horizon, the participants' willingness to change the current level of investment in a risky but viable project significantly dropped as the frequency of monitoring increased, even though additional investment would enhance the likelihood of the project's eventual success—a dysfunctional effect. We also observed that more frequent monitoring significantly decreased the willingness of managers to continue with a risky but viable project regardless of incentive horizon and the effect was significantly pronounced in the presence of a short-term, relative to long-term, incentive horizon—another dysfunctional consequence. Implications of the research findings to theory and practice are discussed.

Keywords: *continuous monitoring; incentive compensation; earnings management; project continuation.*

Data Availability: *Contact the authors.*

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I. INTRODUCTION

Organizations are increasingly implementing continuous monitoring within the internal audit function, as evidenced by a recent survey wherein 81 percent of organizations reported that their internal auditors either have implemented or plan to implement continuous monitoring (PWC 2006).[1] Continuous monitoring processes use automated routines to examine accounting transactions and to report unusual variations, exceptions, and control violations on a (near) real-time basis (Coderre 2006). According to agency theory, more investment in monitoring should reduce the likelihood of agents engaging in opportunistic behavior by alleviating information asymmetry between the agent and principal (Jensen and Meckling 1976; Baiman 1990). We suggest that continuous, relative to periodic, monitoring will diminish managers' temptations to engage in certain dysfunctional behaviors, such as earnings management of real activities, because they will realize the difficulty of justifying such behaviors to their superiors.[2] However, continuous monitoring can also increase managers' apprehension over their performance evaluations and make them feel as though their superiors do not trust them, ultimately leading to greater risk aversion when making risky decisions on behalf of the firm (Baiman 1990; Wiseman and Gomez-Mejia 1998).

Prior literature suggests that researchers should examine the joint impact of monitoring and incentives because the two organizational control mechanisms are intertwined (Chow et al. 1995). Hence, our research objective is to examine the extent to which continuous monitoring can potentially interact with long-term and short-term performance-contingent incentive horizons to yield potential functional and dysfunctional effects on managerial decisions.

Contingent incentive schemes are designed to direct agents' efforts toward principals' goals (Eisenhardt 1989); however, short-term performance-based incentives can activate dysfunctional behavior in the form of discretionary earnings management of real activities (Guidry et al. 1999). We predict that more frequent monitoring can dampen earnings management behavior of this nature in the presence of short-term incentives—a functional effect. However, we also posit that more frequent monitoring will increase agents' risk aversion, which will become manifest in their reluctance to increase the current level of investment in a long-term project regardless of incentive horizon and their reduced willingness to continue with a long-term project particularly in light of a short-term incentive horizon—dysfunctional effects.

To test our predictions, 72 experienced corporate managers participated in a two (monitoring frequency: periodic or continuous) by two (incentive horizon: short-term or long-term) randomized, between-participants experiment. Consistent with expectations, earnings management behavior was reduced with continuous monitoring compared to periodic monitoring in the presence of short-term incentives. Further, two predicted dysfunctional effects of continuous monitoring were also observed: (1) in the presence of long-term incentives, continuous monitoring significantly lowered agents' willingness to increase the current level of investment in a risky project that could potentially yield long-term gains for the organization; and (2) in both incentive horizons, continuous monitoring significantly decreased the willingness of managers to continue with a viable ongoing project, and this effect was significantly magnified in the short-term horizon condition.

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[1] For clarity, we distinguish continuous monitoring from continuous auditing. In theory, the concept of auditing is a subset of monitoring (Jensen and Meckling 1976). By extension, continuous auditing is a subset of continuous monitoring. Continuous monitoring provides external auditors, internal auditors, and corporate managers the capability to track, in (near) real time, financial and nonfinancial information flowing through a company's information systems. The American Institute of Certified Public Accountants (AICPA)/Canadian Institute of Chartered Accountants (CICA) define continuous auditing in reference to external audit reports (AICPA/CICA 1999). The Institute of Internal Auditors (IIA) defines continuous auditing to include internal audit activities (Coderre 2006). To avoid conflict, we refer to the broader theoretical construct of continuous monitoring.

[2] The current study considers division managers as agents and upper managers as principals. Jensen and Meckling (1976) and Lambert (2001) indicate that the economic principles of agency theory can apply across all levels of organizations.

The current study contributes to managerial accounting research and agency theory by demonstrating how more frequent monitoring and contingent incentive horizons can interact to affect agents' decisions in beneficial and harmful ways. The study also enhances audit and accounting information systems research by demonstrating potential behavioral ramifications that should be considered when implementing continuous monitoring and assurance technologies into organizations. From a practice standpoint, one of the main obstacles and challenges to widespread adoption of more frequent monitoring and auditing is obtaining managerial support (Hunton et al. 2003), and our findings suggest that the lack of such support arises from both behavioral and economic sources.

The remainder of this paper is organized as follows. Section II provides background on continuous monitoring and develops research hypotheses. Sections III and IV present the experimental method and results, respectively. Section V provides conclusions.

II. BACKGROUND AND HYPOTHESES

The Institute of Internal Auditors (IIA) argues that increasing regulatory pressures, globalization of businesses, market pressure to improve operations, and rapidly changing business conditions create the need for more frequent and timely monitoring procedures to provide assurance that controls are working effectively and risk is being mitigated (Coderre 2006). They recommend continuous monitoring as a solution. The IIA defines continuous monitoring in terms of the processes put into place by management to continually ensure that policies, procedures, and business processes are operating effectively. Continuous monitoring involves establishing automated tests to highlight system activities or transactions that fail to comply with control objectives (Coderre 2006).

AT&T Bell Laboratories developed one of the earliest prototype systems in the area of more frequent monitoring (Vasarhelyi and Halper 1991). The prototype system, meant to assist internal auditors, was based on the concept of integrating automated monitoring routines into the accounting information system so that transactional data flowing through the system could be continuously evaluated.[3] The IIA (Coderre 2006) describes more current continuous monitoring processes as involving the automated testing of all transactions and system activities (such as changes to automated control settings) within a given business process area, against a suite of control rules and parameters that carefully define threshold levels of exception. Those transactions or system activities that exceed the threshold are flagged as control exceptions. Depending on the level of severity, internal auditors or different levels of management are notified of exceptions, generally by email. The required response may be further investigation, correction of defective transactions, or remediation of control deficiencies. Use of continuous monitoring techniques within organizations has been growing rapidly, particularly since the Sarbanes Oxley Act of 2002 (SOX) required management to document and test internal controls (PWC 2006).[4] This study examines how the use of continuous monitoring routines can affect the decisions of the internal agents who are monitored.

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[3] Other early developments of continuous monitoring techniques include Koch (1981) and Groomer and Murthy (1989). For a review of research on the development of continuous monitoring techniques see Chou et al. (2007).

[4] PWC (2006) notes that "continuous" does not always mean constant, but ranges on a continuum from constant to hourly, daily, or even monthly routines.

Agency Theory and Managerial Decision Making

The principal-agent model assumes that agents are risk- and effort-averse, and will act in a self-serving manner when opportunities arise (Baiman 1990). Agency problems, such as moral hazard, can arise when an agent's and a principal's economic goals are incongruent, and the agent holds an information asymmetry advantage over the principal (Jensen and Meckling 1976; Baiman 1990). Eisenhardt (1989) indicates that the two primary control mechanisms used to address agency problems are incentive compensation contracts, designed to align both parties' economic interests, and information systems, designed to monitor the agent's actions and achieve greater information symmetry between the two parties. This study develops theoretical predictions regarding these two control mechanisms in a continuous monitoring context.[5]

The decision context of the current study focuses on division managers' intratemporal investment in, and willingness to continue, a long-term risky project. The term "intratemporal" is derived from economics and refers to periodic decisions to allocate scarce resources to long-term risky investments as time unfolds (Anderson 1991). Commensurate with such long-term investment decisions where a division manager is making intratemporal allocation decisions on behalf of a firm, the manager (agent) is expected to absorb some investment risk (Cohen et al. 1981). Thus, the agent's incentives should be aligned with, and designed to encourage, such risk-taking (Dittmann and Maug 2007). We first consider prior research that establishes how the incentive horizon can align the economic goals of an agent and principal such that the agent is willing to assume some investment risk. We then consider expected effects of monitoring frequency, and develop interaction hypotheses between incentive horizon and continuous monitoring.

Incentive Horizon

Integrating a contingent component into a compensation contract is often used to align the economic goals of agents and principals (Eisenhardt 1989; Baiman 1990). Such contingencies can be based on firm performance, stock price valuations, investment yields, and other outcomes. While contingent profit-sharing incentives reflect a typical component of a division manager's compensation scheme in practice (Chow et al. 1995; Guidry et al. 1999), a common form of contingent compensation (profit-sharing based on short-term outcomes) has been linked to inappropriate agent behavior. For instance, short-term profit-sharing compensation has been associated with managers misrepresenting their division's expected profitability to upper management (Waller 1994; Chow et al. 1995, Chow et al. 2000). In addition, Guidry et al. (1999) suggest that division managers with short-term contingent incentives will seek to maximize their personal wealth by aligning the division's short-term reported earnings with contingent-based benchmarks. Thus, division managers who are compensated with short-term bonuses that are based on increasing the division's financial performance from one period to the next have been found to record income-increasing discretionary accruals in the current period.[6]

Hoskisson et al. (1993) found a negative association between R&D expenditures and incentives that were based on annual firm performance for division managers. They also reported that incentives based on longer-term firm performance attenuated the negative association. This behavior is referred to as *earnings management of real activities* because current expenditures that are not essential to short-run profitability are reduced, even though such expenditures may be essential to long-run firm efficacy (Roychowdhury 2006).

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[5] The use of agency theory to understand behavioral issues is well established in the accounting literature. For instance, agency theory has served as the theoretical foundation in studies that have examined management audits (e.g., Chow et al. 1995; Chow et al. 2000), internal audits (e.g., Churchill et al. 1982; Tosi et al. 1997; Anderson and Young 1988), and external audits (e.g., Ng and Stoeckenius 1979; Fischbacher and Stefani 2007).

[6] Prior research findings that short-term contingent incentives increase earnings management behavior for division managers are consistent with a larger body of research that reports similar findings for top executive behavior (Richardson and Waegelein 2002; Gaver et al. 1995; Healy 1985; Healy and Wahlen 1999; Holthausen et al. 1995). We emphasize research at the division manager level to correspond with our experimental level of analysis.

The current study examines a real earnings management technique (Hoskisson et al. 1993) in an intratemporal investment in a long-term risky project setting. Consistent with Hoskisson et al. (1993), we expect that managers whose contingent incentive is based on an increase in the firm's short-term (long-term) financial performance will decrease (increase) the firm's existing level of intratemporal investment in a risky long-term project. A decrease in the current level of investment is consistent with earnings management of real activities, i.e., an attempt to maximize short-term contingent incentives by decreasing expenses in the current period. Alternatively, an increase in current investments is consistent with managers attempting to maximize long-term performance, hence, increasing the likelihood of obtaining their long-term incentives.

Another potential impact of short-term and long-term contingent incentive horizons focuses on how such horizons can influence a manager's willingness to take risks. Yates and Tinius (1998) found that long-term incentive compensation schemes for responsibility centers help align division managers' interests with the firm's planning horizon. They examined the adoption of potentially positive net present value investments and found that short-term (long-term) incentive compensation tends to discourage (encourage) such adoption. Consistent with Yates and Tinius (1998), we also expect that managers will be more willing to continue with a risky long-term project when compensated with long-term incentives relative to short-term incentives.

Monitoring Frequency

Monitoring is a control mechanism used to reduce the opportunity for an agent to engage in dysfunctional behavior by alleviating information asymmetry between the agent and principal (Jensen and Meckling 1976; Baiman 1990). As Eisenhardt (1989, 60) stated: "since information systems inform the principal about what the agent is actually doing, they are likely to curb agent opportunism because the agent will realize that he or she cannot deceive the principal." Examples of information systems that are used for monitoring purposes include budgeting systems, reporting procedures, direct supervision, and internal auditing (Jensen and Meckling 1976; Eisenhardt 1989).

Internal audit research indicates that increased monitoring indeed reduces inappropriate behavior. Churchill et al. (1982) found that internal audits reduced noncompliance with rules and operating procedures. Their results also suggested that even the anticipation of an internal audit reduced such inappropriate behavior, though not as much as an actual audit. Anderson and Young (1988) demonstrated that as internal audit increased the probability of detection of misappropriation, expected losses to the organization from misappropriation decreased because managers desired to avoid various penalties, e.g., loss of reputation and compensation.

Monitoring by internal audit is traditionally conducted on a relatively infrequent basis, often rotating among divisions over periods of time. Further, due to unforeseen future circumstances, plans for periodic audits often change such that the actual occurrence of a scheduled audit is somewhat uncertain (Anderson and Young 1988). Continuous monitoring is performed automatically through a computerized system; hence, the actual occurrence of monitoring is certain (Vasarhelyi and Halper 1991).[7] Because earnings management through manipulation of either accruals or real activities is considered inappropriate (Roychowdhury 2006) and managers will likely find it difficult to defend such actions, we expect to find a functional effect of continuous, relative to periodic, monitoring—the reduction of real earnings management behavior.

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[7] Predictable continuous monitoring by computer systems does not suggest that internal auditors are constantly and passively waiting for the system to flag an exception or deviation. Rather, such exceptions and deviations are typically reported via email or a similar type of notification mechanism, and the internal auditors attend to them in accordance with their schedules, and the severity of the exceptions and deviations.

However, there is also a potential unintended dysfunctional consequence of continuous monitoring of managers' intratemporal investment in a risky project. We propose that managers might be unwilling to risk increasing intratemporal investments, even when evidence suggests that such an increase might improve the probability of achieving a successful project outcome. The agent's reluctance to increase the current level of investment is rooted in evaluation apprehension and risk aversion.

Baiman (1990) suggests that the principal is not always better off with more monitoring because the principal typically cannot commit to the agent *ex ante* how the information will be used; thus the agent likely will anticipate that the principal will use the information opportunistically. Such anticipation can result in agents becoming apprehensive about their evaluations and feeling distrusted by their superiors. Evaluation apprehension triggered by continuous monitoring might not be unfounded. Frederickson et al. (1999) demonstrated that a supervisor's *ex post* knowledge of outcomes can affect his/her evaluation of the subordinate who made the judgment (i.e., hindsight bias), and further observed that more frequent reports of outcomes can exacerbate this bias.

For a task, such as that reflected in the current study, that involves a decision for which the outcome is not objectively determinable in the present or immediate future, increased monitoring can heighten risk aversion (Wiseman and Gomez-Mejia 1998). Increased risk aversion by agents can lead them to be deliberately cautious. The agent may decide that if a risky decision is made, based on sound analysis of the facts at hand at the time of the decision, but the outcome ends up to be negative, then hindsight bias could negatively impact the principal's evaluation of the agent. As a result, not only will agents be unwilling to change the *status quo* with regard to an intratemporal investment decision when monitoring is more frequent, but we also expect that more frequent monitoring will dampen their willingness to continue with a long-term risky project. Wiseman and Gomez-Mejia (1998) note that increased monitoring does not increase risk aversion for objectively determinable decisions because the agent and the principal can commit *ex ante* to how the information will be used.

Interaction Effects of Incentive Horizon and Monitoring Frequency

Prior research indicates that incentive compensation and monitoring mechanisms can interact. For instance, Chow et al. (1995) found that a profit-sharing compensation scheme interacted with a probabilistic management audit such that incentive compensation was more effective at inducing division managers to report truthfully to upper management when used in conjunction with monitoring. Further, they reported that neither incentives nor monitoring alone was able to eliminate division managers' misrepresentation. We extend prior research in this area by examining the potential joint effects of monitoring frequency and incentive horizon on managerial decision making in a long-term risky project context.

Regarding intratemporal investment in a long-term risky project, the expected influence of incentive horizon is that managers with short-term incentives will make decisions that are consistent with earnings management of real activities, whereas managers with long-term incentives will make decisions that are consistent with increased investment in a long-term risky project. Further, the expected influence of monitoring frequency is that more frequent monitoring will dampen both behaviors. Hence, when incentive horizon and monitoring frequency are coupled, we predict that with continuous monitoring, where we assume the agent is more risk-averse, managers will be inclined to leave the current level of investment in the project unchanged (maintain the *status quo*), regardless of incentive horizon. With periodic monitoring, a short-term incentive horizon will result in a decrease in intratemporal investment (consistent with real earnings management), while a long-term incentive horizon will yield an increase in intratemporal investment. Hence, we predict the following interaction:

> **H1:** The differential effect of long- versus short-term contingent incentives on managers' willingness to change the dollar amount of intratemporal investment in a risky project will be smaller in the presence of continuous, relative to periodic, monitoring.

Finding empirical support for this interaction hypothesis would demonstrate both functional and dysfunctional effects of continuous monitoring on intratemporal investments in long-term risky projects. Specifically, continuous monitoring complements short-term incentives by reducing inappropriate behavior associated with earnings management of real activities—a functional effect; however, continuous monitoring reduces the effectiveness of long-term incentives that are designed to encourage healthy risk-taking behaviors—a dysfunctional effect.

Regarding risky long-term project continuation, the expected influence of incentive horizon anticipates that managers with short-term incentives will be less willing to continue a risky long-term project. The expected effect of monitoring frequency anticipates that managers with continuous monitoring will be less willing to continue the project. When the interaction of monitoring frequency and incentive horizon is considered, we expect that in the presence of periodic monitoring, managers will exhibit a relatively strong willingness to continue with a risky project regardless of incentive horizon, as the relative infrequency of monitoring will encourage long-term risk-taking. In the presence of continuous monitoring, managers with long-term incentives will be moderately willing to continue with the project because, even though their risk aversion is higher than under periodic monitoring, their long-term incentives will offer an offsetting motivation to continue the project. Managers with short-term incentives and continuous monitoring, however, will be unwilling to continue with a risky long-term project because they are more risk-averse and their short-term incentives offer them no offsetting motivation. Accordingly, our second interaction hypothesis is as follows:

> **H2:** The differential effect of long- versus short-term contingent incentives on managers' willingness to continue with a long-term risky project will be larger in the presence of continuous, relative to periodic, monitoring.

III. RESEARCH METHOD

To test our hypotheses, we administered a 2 × 2 between-participants experiment. The two randomly assigned treatments were monitoring frequency (periodic or continuous) and incentive horizon (short-term or long-term). Each participant read a project management case and made two decisions: whether to change current expenditures related to the project and whether to recommend continuing or discontinuing the project. Participants were asked to assume the role of a division manager at a publicly traded technology manufacturing company.[8]

The case first presented background information about the company. The case company has approximately $1 billion in assets and reports approximately $90 million in net income in 2005. Division managers actively participate in selecting, managing and evaluating investments in various projects for their division, and report to the chief operating officer (COO). The company's primary measure used to evaluate performance is return on investment (ROI). To evaluate the viability of projects, ROI is compared to the company's cost of capital.

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[8] The case setting was based on Kadous and Sedor (2004). Note the following differences between the two studies: (1) our case involved project continuation, whereas their case involved project discontinuation; (2) they presented the experimental data to their participants in terms of dollars, whereas we used percentages; and (3) they did not investigate continuous monitoring.

Independent Variables

The monitoring treatments were operationalized as follows:

> The company's internal audit department audits divisions on a **continuous (rotating)** basis such that divisions are audited **at all times (once every three years). On a continual basis, automated software collects (During an audit, the auditors collect)** actual and projected information on projects from the system's database and tests transactions for unexpected variations, errors and control violations. Specifically, sales and production costs are compared against projections, and recorded spoilage and quantities returned due to defects are examined. Significant variances and control exceptions are **continuously reported as they occur (reported whenever the audit is complete)** to the divisional managers, the COO, and other corporate management.

The periodic monitoring treatment is patterned as a traditional internal audit with uncertainty about the ultimate occurrence and timing of the audit (Anderson and Young 1988). The continuous monitoring treatment reflects a transactions-based audit with alerts for exceptions, one of the two most common applications of continuous monitoring techniques by internal audit departments (Krell 2006; Alles et al. 2006; Coderre 2006).[9] Transactions-based continuous monitoring is most appropriate for this study because it is the type most likely to detect actual decisions made by division managers (Krell 2006). The continuous monitoring procedures are a variation of exception reporting where the automated testing of all transactions against a set of controls rules allows certain transactions to be flagged as control exceptions and management notified (Coderre 2006). The same details of sales revenues and production costs including spoilage and returned goods are monitored in either case, but in the continuous monitoring treatment, reports of exceptions would be sent to upper management right away while in the periodic treatment a summary report is sent at the end of the periodic audit, whenever that occurs, although the specific timing of the periodic report is uncertain.

The incentive horizon treatments were operationalized as follows:

> As a divisional manager you are paid a fixed salary and a bonus based on your division's actual **annual (three-year moving average)** ROI compared to your budget ROI. Actual **annual (three-year moving average)** ROI is adjusted for any audit findings. Budget ROI is based on expected ROI. Bonuses start when you meet expected **annual (three-year moving average)** ROI and are maximized when you exceed expected **annual (three-year moving average)** ROI. You can earn up to 30 percent of your annual salary in bonuses.

Both short- and long-term treatments reflect prior research findings that corporate incentive plans are generally based on some form of earnings. Our short-term incentive is modeled on Guidry et al.'s (1999) finding that short-term bonuses for division managers are usually based on annual performance, while our long-term incentive is modeled on Richardson and Waegelein's (2002) finding that long-term bonuses are generally based on performance over three to five years. We explicitly connect monitoring to rewards by indicating that actual ROI is adjusted for any audit findings.[10]

The case then presented the decision context. Under the purview of the division manager is a project for the production of fingerprint scanners, a biometric device allowing for secure access to desktop and laptop PCs. The project began in 2004, with 2006 as the current year of the case. To avoid a potential escalation of commitment confound, the case indicated that the project was recently re-assigned to the division manager because the original project manager left the company earlier in the year. The case also indicated that the project would have a significant impact on the division's overall ROI performance.

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[9] The other common form implemented is continuous monitoring of control settings in automated systems (Krell 2006).

[10] Although both the monitoring frequency and incentive horizon treatments could have been operationalized in other ways, the intent is to select treatments that will provide a clear contrast for testing predictions. Pilot testing with Ph.D. students and undergraduates indicated that experimental manipulations effectively triggered internalization of differences in monitoring frequency and incentive horizon.

From its inception in 2004 through the first half of 2006, the fingerprint project has experienced several problems. The product's defect rate has been higher than projected; the product's price has been too high to be competitive; and the return on investment (ROI) has been lower than projected and below cost of capital. However, there are significant indications that these problems are being solved and that the product might eventually exceed the cost of capital in the future. The case also included a discussion of the impact of changing quality control expenditures and revised projected ROI based on either increasing or reducing such expenditures. Thus, participants received both positive and negative information about the fingerprint scanner project and its future prospects.

Dependent Variables

After reading the case, participants made two decisions related to the project. The first decision represents an intratemporal investment decision—whether and to what extent to change the current level of quality control expenditures. Because quality control expenditures are discretionary, managers can use them to manage earnings upward by reducing such expenditures. On the other hand, expenditures could be increased, thus reducing current-year earnings but potentially improving longer-term results by better identifying production problems. A full range (reduce to increase expenditures) is used to reflect the conflict between short-term and long-term performance. Either choice would be reflected in the audit as a variance from budget.

The second dependent variable represents a risky project continuation decision. The project is not performing up to expectations currently, but is expected to exceed cost of capital in future years. Hence, this decision again represents a conflict between short-term and long-term performance. Case analysis of overall expected value compared to cost of capital suggests that, though the project is risky, it should be continued. Participants are requested to indicate their willingness to continue or discontinue the project.

Experimental Procedures

Participants were recruited from educational sessions held by an international consulting firm. The topic of the sessions dealt with documenting and testing accounting internal controls in accordance with SOX. Each session lasted one day. Four sessions were held at four major U.S. cities on four consecutive days. At the beginning of each session, attendees were asked to volunteer to participate in a study about managerial decision-making.[11]

Facilitators (including one of the researchers) handed two sealed envelopes to each participant.[12] Participants opened the first envelope and removed the materials, which included a cover sheet, voluntary consent form, case materials, and dependent variable response sheet. After the participants read the cover sheet and consent form, they reviewed the case materials, responded to dependent variable items, and placed all materials back into and sealed the first envelope. Next, they opened the second envelope, removed the materials within, responded to a series of debriefing questions, provided demographic information, and placed all open materials back into and sealed the second envelope. After sealing the first envelope, the facilitator ensured that participants did not reopen it while the second envelope was open. The researchers provided each participant with a $50 contribution to the charity of his/her choice as an incentive to participate.

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[11] The educational session dealt with how to document management's evaluation of internal controls throughout the enterprise. There was no discussion of continuous monitoring, as the focus was on documentation techniques. The research study was administered before the educational session began to ensure that there would be no potential confound between the experiment and the session topics.

[12] The experimental treatments were stacked in random order.

IV. RESULTS

Participants

A total of 72 corporate managers participated in the study, each representing a different company.[13] Table 1 presents demographic information on the participants. As shown, participants' mean age was 40.57 years, mean years of total business experience was 16.38, and mean years in current position was 8.79. There were 53 male (74 percent) and 19 female (26 percent) managers in the sample. Seventy-five percent of the managers declared a bachelor's degree as their highest educational level, while the remaining also obtained a master's degree. The participants assessed their managerial levels as middle (43 percent) or upper (57 percent) management, and described their job function as general management (54 percent) or finance/accounting/internal audit (46 percent). The participants' companies represented a variety of industries, with the largest percents in manufacturing and retail (35 percent and 26 percent, respectively). Finally, the managers self-assessed the size of their companies within their industries as large (54 percent), medium (38 percent) or small (8 percent).[14] In summary, the participants are experienced managers representing a variety of industries and firm sizes.

Manipulation Checks

After the main experiment, participants completed questions designed to indicate whether they had read the case and internalized the treatments. Regarding the manipulation for monitoring frequency, participants were asked whether the internal audit department's approach to monitoring performance was on a rotating basis over three years or on a continuous basis. All participants correctly answered this question. In addition, participants rated the company's monitoring system on an 11-point scale[15] anchored by "provides too much scrutiny" on one end (coded as negative numbers) and "provides too little scrutiny" on the other end (coded as positive numbers), with the middle (0) indicating "neither." The means in continuous and periodic monitoring treatments were −3.91 and 2.61, respectively. The means were significantly different ($t_{70} = 28.82$, $p < 0.01$), indicating that participants believed that continuous monitoring provided greater scrutiny than periodic monitoring, as intended.

Regarding the incentive horizon treatment, participants were asked whether the basis for determining their bonus was annual ROI or three-year moving average ROI. All participants responded correctly. Participants also rated the bonus compensation system on an 11-point scale anchored by "encourages managers to focus on short-term ROI" on one end (coded as negative numbers) and "encourages managers to focus on long-term ROI" on the other end, with "neither"

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[13] All companies are listed on a major U.S. stock exchange.

[14] Using Chi-square matrices to test the categorical demographic variables and regression models to test the continuous demographic variables, statistical results were not significant across experimental treatments (all $p \geq 0.10$); thus, the randomization procedure was deemed effective. Further, the demographic variables were included as covariates in an ANCOVA model, but all were nonsignificant (p > 0.10) and did not change the results related to the manipulated variables. Hence, they are not shown in the subsequent analyses.

[15] Unless otherwise noted, all scales were centered on 0, with the numbers 1 to 5 shown on each side of 0. For purposes of statistical analyses, we coded one side of each scale as negative numbers. The negative signs did not appear in experimental materials provided to participants.

TABLE 1

Demographic Profile of 72 Participants

	Mean	**Std. Dev.**	**Minimum**	**Maximum**
Age	40.57	6.10	29	51
Years of professional work experience	16.38	6.08	5	27
Years in current position	8.79	5.67	6	23
	Number	**Percent**		
Gender				
Male	53	74%		
Female	19	26%		
Highest degree obtained				
Bachelor's	54	75%		
Master's	18	25%		
Managerial level of current position				
Middle manager	31	43%		
Upper manager	41	57%		
Description of current job function				
General management	39	54%		
Finance/accounting/internal audit	33	46%		
Industry of current employer (two-digit SIC code)				
Manufacturing	25	35%		
Retail	19	26%		
Transportation	16	22%		
Financial services	7	10%		
Mining/oil/gas	4	6%		
Health care	1	1%		
Size of current employer (within industry)				
Large	39	54%		
Medium	27	38%		
Small	6	8%		

used as the mid-point (0). The means for short-term and long-term were −3.59 and 2.61, respectively. The means are significantly different ($t_{70} = 25.29$, $p < 0.01$), suggesting that the participants internalized incentive compensation as intended. We also asked participants to rate the bonus compensation system on an 11-point scale anchored on "discourages managers from working hard" and "encourages managers to work hard" with "neither" as the mid-point to determine whether they recognized the general intended effects of incentive compensation. The mean response of 3.17, on the "encourages managers to work hard" end of the scale, indicates that the participants believed the bonus was a motivator to work hard. The variable was not significantly different across treatment conditions ($p \geq 0.10$), providing assurance that any incentive horizon effects will not be confounded with differential effort related to the bonus compensation system.

Finally, we administered two manipulation checks related to the participants' degree of commitment to the project. We were concerned that the participants' commitment to the project might vary across conditions, thus confounding an escalation of commitment phenomenon with the experimental treatments. To avoid this possible confound, the case materials stated that the project, already in progress, had been reassigned to the division manager in the current year. All participants correctly answered the question as to whether the project was re-assigned to them. A second question asked the participants to rate the extent to which they felt personally committed to the project on an 11-point scale anchored on "not at all committed" and "completely committed." The overall mean for this item was 3.20, and results of ANOVA testing indicated nonsignificant monitoring ($F_{1,68} = 2.35$, $p = 0.13$), incentive ($F_{1,68} = 0.01$, $p = 0.92$), and interaction effects ($F_{1,68} = 1.36$, $p = 0.25$). Overall, test results suggest that participants' commitment to the project did not vary across treatment conditions.

Hypothesis Testing

In Table 2, Panel A shows the coded response scale for the dependent variable "willingness to change quality control expenditures," Panel B presents descriptive statistics by treatment condition, Panel C offers the results of ANOVA model testing, and Panel D reflects the results of planned contrast testing for H1. Graphical results of dependent variable responses are shown on Figure 1. As shown in Panel B, the main effect means in the periodic monitoring and continuous monitoring conditions were 0.48 and 0.43, respectively; as indicated in Panel C, the means are not significantly different ($p = 0.94$). Also as shown in Panel B, for participants with short-term incentives, the main effect mean response was to decrease quality control expenditures (−0.88) and for participants with long-term incentives, the main effect mean response was to increase quality control expenditures (1.66); Panel C indicates that this incentive main effect is significant ($p < 0.01$). The interaction term in Panel C is also significant ($p < 0.01$), as next interpreted.

Hypothesis 1 predicts that the differential effect of long- versus short-term incentives on managers' willingness to change quality control expenditures will be greater in the periodic, relative to the continuous, monitoring condition. As shown in Table 2, Panel B, the mean willingness to change quality control expenditures in the periodic monitoring treatment is 2.50 (−1.65) in the presence of long-term (short-term) incentive horizon. In the continuous monitoring treatment, the means are 0.90 (−0.12) in the presence of long-term (short-term) incentive horizon. Figure 1 indicates a pattern of results that is consistent with H1. We ran a planned contrast of simple effects (Keppel and Wickens 2007) to further test H1, as shown in Table 2, Panel D. The difference in willingness to change the quality control expenditures between managers with long-term and short-term incentive horizons is significantly greater ($p < 0.01$) in the presence of periodic than continuous monitoring, supporting H1.

In Table 3, Panel A shows the coded response scale for the second dependent variable "willingness to continue or discontinue the project," Panel B presents descriptive statistics by treatment condition, Panel C offers the results of ANOVA model testing, and Panel D reflects the results of contrast testing. Graphical results of dependent variable responses are shown on Figure 2. As indicated in Table 3, Panel B, the main effect monitoring means are 3.40 in the periodic monitoring condition and −0.81 in the continuous monitoring condition; Panel C indicates that the monitoring main effect is significant ($p < 0.01$). As displayed in Table 3, Panel B, the incentive main effect means in the long-term and short-term conditions are 2.47 and −0.15, respectively; Panel C shows that the incentive main effect is significant ($p < 0.01$). Finally, Table 3, Panel C, indicates a significant interaction term ($p < 0.01$), which is discussed next.

Hypothesis 2 predicts that the differential effect of long- versus short-term incentives on managers' willingness to continue the project will be greater in the continuous, relative to the

TABLE 2
Willingness to Change Quality Control Expenditures

Panel A: Coded Response Scale for Purposes of Statistical Analyses

-5	-4	-3	-2	-1	0	1	2	3	4	5
Definitely Reduce Quality Control					No Change					Definitely Increase Quality Control

Panel B: Descriptive Statistics—Mean (Standard Deviation) [Sample Size]

	Incentives		
Monitoring	**Long-Term (LT)**	**Short-Term (ST)**	**Overall Monitoring**
Periodic	2.50 (1.95) [18]	−1.65 (2.34) [17]	0.48 (2.98) [35]
Continuous	0.90 (2.07) [20]	−0.12 (2.06) [17]	0.43 (2.10) [37]
Overall Incentives	1.66 (2.15) [38]	−0.88 (2.31) [34]	0.45 (2.55) [72]

Panel C: ANOVA Model of Between-Subjects Effects

Source	S.S	d.f.	M.S.	F-Ratio	p-value (two-tailed)
Monitoring	0.02	1	0.02	0.01	=0.94
Incentives	119.51	1	119.51	26.91	<0.01
Monitoring * Incentives	43.88	1	43.88	9.88	<0.01
Error	301.95	68	4.44		
Total	461.88	71			

Panel D: Contrast Testing for Interaction H1 (two-tailed p-values)

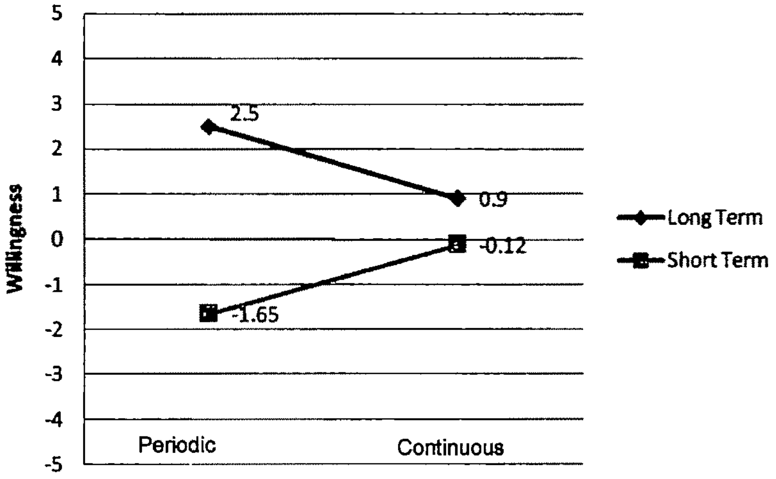
H1: (Periodic LT−Periodic ST) > (Continuous LT−Continuous ST)	t-statistic	p-value
[2.50−(−1.65)] > [0.90−(−0.12)]	3.14	<0.01

periodic, monitoring condition. As shown in Table 3, Panel B, the mean willingness to continue the project in the periodic monitoring treatment is 3.94 (2.82) in the presence of long-term (short-term) incentive horizon. In the continuous monitoring treatment, the means are 1.15 (−3.12) in the presence of long-term (short-term) incentive horizon. Figure 2 illustrates a pattern of results that is consistent with H2. We further test H2 with a planned contrast of simple effects. Table 3, Panel D, shows that the difference in willingness to continue the project between managers with long- and short-term incentive horizons is significantly greater ($p < 0.01$) in the presence of continuous than periodic monitoring, supporting H2.

Post Hoc Analysis

The experiment incorporated two decisions into one context to demonstrate potential functional and dysfunctional effects arising from an interaction between incentive horizon and monitoring frequency. In additional analysis (not tabulated), we tested for independence between these

FIGURE 1
Graphical Depiction of "Willingness to Change Quality Control Expenditures"[a]



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[a] Dependent Variable Coding: (−5 = Definitely Reduce, 0 = No Change, +5 = Definitely Increase)

two decisions by including the task decision that occurred first in time ("willingness to change quality control expenditures") as a covariate in an analysis of the task decision that occurred second ("willingness to continue or discontinue the project"). Though the decision to change quality control expenditures is significant ($p = 0.04$) in the ANCOVA model, the main and interaction effects of monitoring and incentives remain highly significant and the overall model R^2 does not significantly increase ($p > 0.10$), suggesting that the results for project continuation are not totally driven by the quality control decision.

Post-Experiment Debriefing

We collected data, via post-experiment debriefing, to test some key process variables that were suggested in development of our hypotheses. Theory indicates that continuous monitoring, relative to periodic monitoring, increases risk aversion through increased evaluation apprehension and reduced trust by supervisors. Three items in the post-experimental questionnaire addressed these three constructs. We asked participants to assess the impact of the company's monitoring system on each item by marking the appropriate point on an 11-point scale, with "neither" as the mid-point (0). For risk aversion, the endpoints were "discourages risk taking" (−5) and "encourages risk taking" (+5). The mean response in continuous monitoring was −3.70 and the mean response in periodic monitoring was 2.82, and the pattern is consistent with continuous monitoring increasing risk aversion. For evaluation apprehension, the endpoints were "generates anxiety about performance evaluation" (−5) and "alleviates anxiety about performance evaluation" (+5). The mean response in continuous monitoring was −3.51 and the mean response in periodic monitoring was 0.43, again suggesting a pattern that is consistent with continuous monitoring increasing evaluation apprehension. For trust by supervisor, the endpoints were "makes me feel trusted by superiors" (+5) and "makes me feel un-trusted by superiors" (−5). The mean response in

TABLE 3

Willingness to Continue or Discontinue the Fingerprint Scanner Project

Panel A: Coded Response Scale for Purposes of Statistical Analyses

-5	-4	-3	-2	-1	0	1	2	3	4	5
Definitely Discontinue the Project					Not Sure					Definitely Continue the Project

Panel B: Descriptive Statistics: Mean (Standard Deviation) [Sample Size]

	Incentives		
Monitoring	**Long-Term (LT)**	**Short-Term (ST)**	**Overall Monitoring**
Periodic	3.94 (0.87) [18]	2.82 (1.67) [17]	3.40 (1.42) [35]
Continuous	1.15 (2.18) [20]	−3.12 (1.50) [17]	−0.81 (2.86) [37]
Overall Incentives	2.47 (2.19) [38]	−0.15 (3.39) [34]	1.24 (3.10) [72]

Panel C: ANOVA Model of Between-Subjects Effects

Source	**S.S**	**d.f.**	**M.S.**	**F-ratio**	**p-value (two-tailed)**
Monitoring	341.89	1	341.89	126.54	<0.01
Incentives	130.10	1	130.10	48.15	<0.01
Monitoring * Incentives	44.36	1	44.69	16.42	<0.01
Error	183.73	68	2.70		
Total	680.99	71			

Panel D: Contrast Testing for Interaction H2 (two-tailed p-values)

H2: (Periodic LT−Periodic ST) < (Continuous LT−Continuous ST)	**t-statistic**	**p-value**
(3.94−2.82) < [1.15−(−3.12)]	4.05	<0.01

continuous monitoring was −3.68 and the mean response in periodic monitoring was 3.31, indicating lower perceived trust by superiors in the presence of continuous monitoring. ANOVA model results for each measure finds that the main effect for monitoring is significant ($p < 0.01$), the main effect for incentives is not significant ($p > 0.40$), and the interaction term is not significant ($p > 0.18$). These additional analyses provide support for and insight into theorized processes leading to the effects obtained in the experiment related to monitoring frequency.[16]

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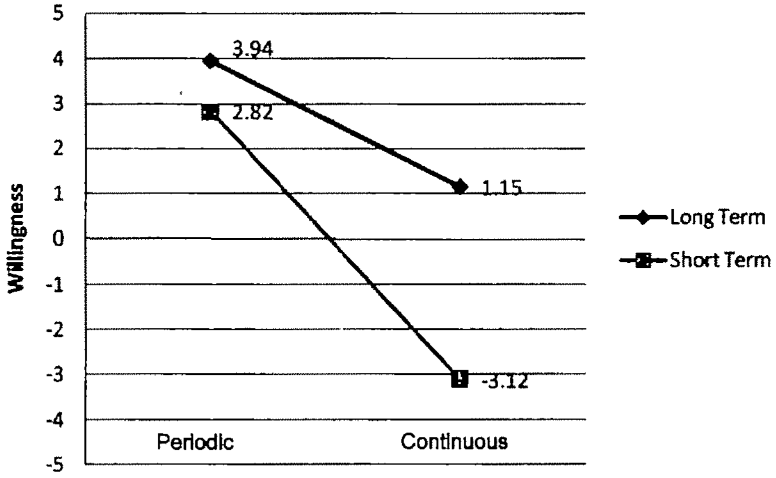
[16] We also performed mediation analyses (not shown) for these process variables, and found that they do not individually mediate the monitoring and incentive effects reported in the main analyses. Since developing and testing a judgment and decision-making model of this nature was not part of our research objective, we are not surprised at the lack of significance because they likely act in combination, and one or more key variables may be omitted. Future research could focus on more precisely mapping the cognitive process through which the implementation of continuous monitoring ultimately leads to increased risk aversion.

FIGURE 2
Graphical Depiction of "Willingness to Continue or Discontinue the Project"[a]



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[a] Dependent Variable Coding: (−5 = Definitely Discontinue, 0 = Not Sure, −5 = Definitely Continue)

V. DISCUSSION

This study examines potential functional and dysfunctional effects of continuous monitoring on resource allocation decisions of managers, conditioned on the horizon over which their contingent incentives are based. The results suggest that with a short-term bonus horizon, continuous monitoring reduced earnings management of real activities, thereby yielding a functional effect. However, the research findings indicate two dysfunctional effects of continuous monitoring, relative to periodic monitoring: Continuous monitoring dampened managers' willingness to invest in a viable but risky project, and continuous monitoring lowered the willingness of managers to continue with the project, particularly in a short-term bonus horizon. Overall, we conclude that the implementation of continuous monitoring in organizations can yield a potential reward to the firm by discouraging certain types of inappropriate behavior, but such benefit might be tied to the unintended consequence of increased risk aversion.

The results of this study suggest that organizations should review existing incentive horizons to determine whether there could be potential functional and dysfunctional interaction effects when they are planning to implement continuous monitoring. If there are potentially conflicting control mechanisms, then organizations will need to carefully weigh the advantages and disadvantages of each mechanism in isolation and in combination to find the most effective set for their circumstances. Organizations can also develop plans to address and overcome any conflicts. These plans might require changing incentive horizons or committing to how continuous monitoring results will and will not be used.

We expect that the extent and form of any changes will likely vary across organizations and circumstances. For example, in organizations in which division managers do not make project continuation decisions but do make decisions that hold discretionary earnings management potential, continuous monitoring can alleviate negative effects of short-term bonuses. In organizations in which division managers make project investment and continuation decisions, the implementation of continuous monitoring will likely require commensurate changes related to incentive compensation. One approach is to consider implementing long-term incentives across all compensation plans, though Shelley and Omer (1996) caution that this approach might require higher levels of incentives to compensate for lengthening the time horizon. An alternative approach might consider utilizing a balanced scorecard that incorporates nonfinancial metrics in addition to financial indicators, so that goal-congruent risk-taking is encouraged. Management should also consider extensive communication and training programs aimed at alleviating the negative consequences of continuous monitoring while retaining the positive aspects. Future research in this area should consider these and other possible remedies and their accompanying trade-offs.

One limitation of our study relates to the two decision tasks used in the experimental instrument, i.e., quality control expenditures and project continuation. While these are important tasks commonly confronted by managers, there are other types of managerial tasks in which the interactive effects of compensation incentives and monitoring frequency might differ from those found in this study, such as investment choice or employee evaluations. Another limitation stems from the monitoring frequency and incentive compensation treatments manipulated in the current study. The intensity of both monitoring frequency and incentive compensation varies on a continuum and we selected two levels at opposing ends of each spectrum to test theory. While we recognize that different levels might change the magnitude of results, we see no reason to expect directional changes. Additionally, the negative impact of continuous monitoring may dissipate over time as managers become accustomed to more frequent monitoring (Indjejikian 1999). Future research is needed to examine the sensitivity of our findings to different tasks, monitoring frequencies, incentive structures, and circumstances, as well as the multi-period implications of our results. We also recommend conducting further research to better understand why continuous monitoring dampens earnings management tendencies under short-term incentives. Special attention should be paid to increasing our knowledge of the underlying ethical motivations of managers making the type of investment decisions examined in this study.

The current study augments extant theory, research, and practice on several fronts. We contribute to agency theory by demonstrating that two common mechanisms for alleviating principal-agent friction, monitoring and incentives, can interact and affect human behavior in unintended ways. In managerial research, we extend Chow et al. (1995) by demonstrating that the joint effects of monitoring and incentives can produce dysfunctional effects as well as functional effects, which is consistent with misaligned incentives and monitoring. We extend accounting information systems research on continuous monitoring by examining possible psychological and behavioral ramifications of implementing continuous monitoring in organizations, as called for by Hunton et al. (2003) and Kogan et al. (1999).

Findings from the current study also support similar results of Bhojraj and Libby (2005); that is, in a financial reporting context they report that more frequent *external* reporting appeared to induce suboptimal resource allocation in the presence of exogenous stock market pressure; we broaden their research by demonstrating that more frequent *internal* reporting of continuous monitoring results also can evoke suboptimal resource allocation, primarily triggered by endogenous incentive pressure. Our experimental results hold implications for auditing research on continuous monitoring by either internal or external auditors. Though auditing research generally views continuous monitoring positively, there may be unintended consequences that help to explain managerial reluctance to implement continuous monitoring. Finally, for practice, organizations should carefully consider the potential functional and dysfunctional effects of continuous monitoring when implementing the associated processes and technologies.

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Does Earnings Management Affect Firms' Investment Decisions?

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ABSTRACT: This paper examines whether firms manipulating their reported financial results make suboptimal investment decisions. We examine fixed asset investments for a large sample of public companies during the 1978–2002 period and document that firms that manipulate their earnings—firms investigated by the SEC for accounting irregularities, firms sued by their shareholders for improper accounting, and firms that restated financial statements—over-invest substantially during the misreporting period. Furthermore, following the misreporting period, these firms no longer over-invest, consistent with corrected information leading to more efficient investment levels. We find similar patterns for firms with high discretionary revenues or accruals. Our findings suggest that earnings management, which is largely viewed as targeting parties external to the firm, can also influence internal decisions.

Keywords: *earnings management; investment.*

I. INTRODUCTION

This paper examines whether earnings management affects resource allocation by studying whether firms manipulating earnings make suboptimal investment decisions. We aim to provide evidence on whether accounting misstatements, potentially motivated by compensation targets or capital market expectations, cause distortions in the investment decisions made within firms engaging in the misstatement—a direct cost to investors. The consequences of earnings management are relevant to the decisions made by investors, managers, directors, and regulators, and to date there is little research that addresses the relation between earnings management in external reporting and its effect on internal decisions.

Investment decisions depend on expectations of the benefits of the investment, which in turn depend on expectations of future growth and product demand. Expectations of future growth are

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based on information that includes revenues and earnings. In addition to merely concealing the actual performance during the period, misstated financial results can mask underlying trends in revenue and earnings growth. Thus, overstatements of revenues and earnings are likely to distort expectations of growth by those unaware of the misstatement.

One might conjecture that if management chooses to paint a rosier view for investors in the numbers they report externally, then they would not allow this to influence internal investment decisions. However, it is possible that investment decision makers within the firm believe the misreported growth trend—because they are either over-optimistic or unaware of the misstatement—and invest accordingly. Alternatively, investment decision makers might understand the true state of the firm but choose to over-invest in a high-risk approach to turn around performance.

Regardless of the reason for the over-investment, truthful reporting might have prevented it. Several parties are typically involved in investment decisions, including managers who make the decision to invest, boards who review the capital budget, and external suppliers of capital. If financial results are reported truthfully, then other parties could step in to curtail the investment. As a result, firms invest more than they otherwise would have, and attempts to meet capital market expectations or meet bonus targets, for example, could affect investors, employees, customers, and a broad set of related parties.

Our study provides evidence on whether earnings management affects resource allocation by examining the capital expenditure decisions of three groups of firms alleged to have manipulated earnings—firms investigated by the SEC for accounting irregularities, firms sued by their shareholders for improper accounting, and firms that restated financial statements. These samples comprise firms that have overstated earnings substantially, which gives us greater confidence that we have identified earnings manipulators and that the magnitude of their earnings manipulation could significantly affect investment decisions. Both of these factors are crucial to the power of our tests. However, because of concerns about potential selection bias and to test whether the effect we find generalizes beyond the most severe cases of earnings manipulation, we also use discretionary revenues to identify firms that manipulated earnings.

Our data identifies the period for which manipulation is alleged, and permits us to examine the investment decisions in the years before, during, and subsequent to the manipulation. Under the null hypothesis that earnings management does not result in resource misallocation, firms should exhibit investment levels consistent with their fundamentals. Our study follows an extensive literature that models investment decisions using a linear model relating capital expenditures to investment opportunities. Specifically, these models control for investment opportunities using Tobin's Q and cash from operations. Our tests examine the investment decisions in the manipulation and post-manipulation periods. First, we test whether firms overstating earnings over-invest during the manipulation period. Second, we examine whether over-investment is eliminated once earnings are no longer manipulated. If firms over-invest during the manipulation period for a reason other than the effect of misleading information, then we would not expect a decline in investment (after controlling for declining investment opportunities) when earnings are no longer manipulated.

The findings indicate that firms manipulating earnings do over-invest in the misreporting period. We find significantly greater investment than would be expected based on investment fundamentals. Additional tests using matched control firms suggest that sample firms invest more than they would have had they not overstated their earnings. While sample and control firms exhibit some over-investment before the manipulation period, control firms reduce the level of investment, whereas sample firms continue over-investing during the manipulation period. Finally, the evidence indicates that sample firms curb over-investment following the misreporting period. These findings suggest that an important consequence of earnings management is its effect on firms' investment decisions.

Our tests using measures of discretionary revenues allow us to confirm the main findings and also to investigate further the timing of the earnings management and excess investment. Granger causality tests are consistent with earnings management leading excess investment rather than earnings management occurring in response to (e.g., to cover up) past suboptimal investment.

Taken together, our findings across several approaches using multiple measures of earnings management and excess investment support our hypothesis that earnings management affects firms' investment decisions. We seek to provide additional support for this hypothesis by ruling out alternative explanations for our results. Our evidence does not indicate that over-investment is solely a result of firms obtaining relatively inexpensive external financing because we see similar patterns of over-investment in firms with low and high levels of external financing. Our evidence does not support the hypothesis that firms manipulating earnings over-invest to pool with more successful firms to thereby avoid detection; we find that sample firms invest more than control firms, which is inconsistent with a pooling argument. Finally, our evidence is not consistent with the notion that firms facing poor returns to past over-investment overstate earnings to mask the poor financial results. First, we find that firms continue to over-invest while they manipulate earnings, which is unexpected for a firm already facing poor investment returns. In addition, using Granger causality tests we find that earnings management leads excess investment. While our findings are subject to the caveat that we do not observe the investment decision process directly, we conclude that the most plausible explanation is that earnings management distorts information used by those involved in firms' investment decisions.

These findings contribute to the emerging literature on the role financial statement information plays in investing decisions. Recent studies such as Biddle and Hilary (2006), Verdi (2006), and Bushman et al. (2006) find relationships between properties of accounting information and investment decisions. However, these studies do not address whether intentional distortions in accounting numbers affect investment. Our study also contributes to the literature relating earnings management and resource allocation. Our findings suggest that earnings management can lead to a direct cost to investors in the form of inefficient investments.

The layout of the paper is as follows. Section II discusses related literature, Section III describes our hypotheses, Section IV describes our research design, Section V presents our empirical findings, and Section IV concludes.

II. RELATED LITERATURE

Incentives to Manage Earnings

Prior literature on earnings management examines various alternative hypotheses about why firms manipulate earnings. Motivations for earnings management in these studies include influencing the terms of compensation and debt contracts, influencing regulators, and influencing equity prices.[1] Collectively, this literature suggests that the incentives to manipulate earnings arise in a number of contexts. In this study, we examine a potential consequence of earnings management, regardless of managers' motivations. To date, there has been little focus on how earnings management affects internal decisions, such as capital investment.

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[1] See Healy and Wahlen (1999), McNichols (2000, 2002), and Dechow and Skinner (2000) for overviews of this literature.

Earnings Management and Resource Allocation

The prior literature on earnings management provides limited evidence on whether misreporting to investors results in resource misallocation. As Healy and Wahlen (1999) note, only a small part of the earnings management literature addresses the consequences of earnings management on resource allocation, and the findings of this literature are mixed. One stream of this literature examines whether earnings management contributes to IPO mispricing. For example, Teoh et al. (1998) conclude that earnings management contributes to IPO mispricing. However, Brav et al. (2000) find that the long-run returns of IPOs are similar to those of seasoned firms with similar market capitalization, suggesting the findings may be due to a more pervasive return pattern in the broader sample of public companies.

A second stream of literature, including Foster (1979), Dechow et al. (1996), Beneish (1997), and Palmrose et al. (2004), finds that the market reaction to disclosure of misleading reporting is significantly negative, indicating that investors were not completely aware of the manipulation. Nevertheless, to the extent equity investors have rational expectations of the amount of manipulation occurring, even if they cannot identify the magnitude for specific companies, one could observe negative returns to announcements *ex post* that would not necessarily imply resources were misallocated *ex ante*.

A third stream of literature examines whether firms manipulate real decisions to manage earnings. For example, Bushee (1998) examines how research and development (R&D) spending is affected by incentives to meet earnings targets, and whether this is influenced by the composition of the firm's institutional investors. Another example is the opportunistic timing by banks of sales of held-for-sale securities (Barth et al. 1990). While this stream of literature examines the relation between earnings management and real decisions, the focus is on distorting real decisions to achieve an earnings target. Our study, in contrast, asks whether real decisions are distorted because earnings management results in distorted information for internal decision makers.

Earnings Management and Investment Decisions

Though our study is the first we are aware of to test whether earnings management leads to inefficient investment decisions by providing distorted information to decision makers, other studies have posited and tested theories relating to the link between earnings management and firms' investment decisions. Dechow et al. (1996) study firms targeted by SEC enforcement actions and concludes that a desire to attract external financing at low cost is an important motivation for earnings manipulation. Presumably, these funds are then used for capital investment. Managers considering profitable investment projects but facing financing constraints might manipulate earnings in order to obtain financing for investment. However, it is not clear why these managers would over-invest rather than invest optimally with the funds obtained. Our prediction of over-investment relies on distorted information being used by investment decision makers, rather than just a desire to raise additional capital.

A theoretical paper by Bar-Gill and Bebchuk (2003) predicts that inefficient investment projects will more likely be undertaken by companies that misreported prior to undertaking the project because firms overstating their financial results will be able to obtain cheaper financing. Evidence in support of this hypothesis is found by Wang (2006). Wang (2006) finds that misreporting firms are more likely to over-invest in R&D and stock-financed mergers and acquisitions. Our hypothesis does not preclude cheap financing having an effect on investments—we test whether over-investment occurs regardless of external financing.[2]

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[2] In addition, to the extent that the artificially lower cost of capital is captured by our measure of investment opportunities (Tobin's Q), we control for this in our investment model.

A concurrent study by Kedia and Philippon (2009) predicts that firms overstating their financial results pool with better performing firms to avoid detection. These firms over-invest to mimic firms truthfully reporting stronger results. Our study tests a different hypothesis. Rather than arguing that bad-type firms knowingly replicate the investment decisions of good-type firms, we consider the possibility that at least one of the parties involved in the investment decision is misled by the misstated accounting information. We find that sample firms invest more than matched control firms, which is inconsistent with a pooling argument.

Earnings Quality and Investment Decisions

Our study relates to the emerging literature on the role financial statement information plays in investing decisions. For example, Biddle and Hilary (2006) and Verdi (2006) predict and find that better accounting information reduces information asymmetry between managers and outside suppliers of capital, allowing for more efficient investment. Biddle and Hilary (2006) find that measures of accounting quality are negatively related to investment-cash-flow sensitivities, indicating that the effect of financing constraints on investment is lower for firms with higher accounting quality. Verdi (2006) finds accruals quality is significantly negatively associated with both over-investment and under-investment.

Bushman et al. (2006) argue that timely accounting recognition of economic losses makes managers less likely to engage in *ex ante* negative net present value investment projects. They find that more timely accounting recognition of economic losses curbs over-investment by managers faced with declining investment opportunities—the investment response to declining investment opportunities decreases when loss recognition is more timely. However, these studies do not address whether intentional distortions in accounting numbers affect investment.

III. HYPOTHESES

We test whether earnings management affects investment decisions. Investment decisions depend on expectations of the benefits of the investment, which in turn depend on expectations of future growth and product demand. Expectations of future growth are based on information that includes revenues and earnings. In addition to merely concealing the actual performance during the period, misstated financial results can mask underlying trends in revenue and earnings growth. In fact, Richardson et al. (2002) find that firms that restate financial results tend to be growing firms attempting to report consecutive earnings increases. Thus, overstatements of revenues and earnings are likely to distort expectations of growth by those unaware of the misstatement.

It is possible that investment decision makers within the firm believe the misreported growth trend—due to either their own over-optimism or unawareness of the misstatement[3]—and invest accordingly. Alternatively, investment decision makers might understand the true state of the firm but choose to over-invest in a high-risk approach to turn performance around.[4]

Regardless of the reason for the over-investment, we claim that truthful reporting could have prevented it. Several parties are typically involved in investment decisions, including managers who make the decision to invest, boards who review the capital budget, and external suppliers of capital. Had financial results been reported truthfully, access to capital might have been limited by capital markets or the board of directors, who use financial statements to make capital and monitoring decisions. In addition, investors might have been better able to identify the over-investment, leading to an unwanted decline in the firm's stock price. Thus, earnings management could inhibit disciplining mechanisms that might have precluded over-investment. As managers continue to report strong financial results, there is less opportunity for correction.

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[3] For example, Oracle's aggressive revenue recognition in the early 1990s was in part driven by an aggressive culture and incentives for the sales force, along with weak controls. Their CFO, Jeffrey Henley, noted that a key aspect of turning Oracle around after this period was to slow the business down because their real growth was not as rapid as many within the company had thought (remarks given at Stanford Graduate School of Business, December 1997).

[4] As an example, some view Sunbeam's acquisitions of Coleman, First Alert, and Mr. Coffee as an attempt to generate growth because their management recognized it could not do so from its turnaround efforts (Callan and McNichols 2003).

Our first hypothesis is that firms overstating financial results invest more than they otherwise would have had they reported truthfully; the misstatement of financial results affects investment decisions. Because the earnings manipulations in our samples are largely income-increasing, we hypothesize that the overstatement of financial results leads to over-investment.

Formally stated, our first hypothesis is as follows:

> **H1:** Firms manipulating earnings will have greater investment levels than expected based on the value of their investment opportunities during the period earnings are manipulated.

Our second hypothesis examines the years after the manipulation period to provide supporting evidence that the over-investment in prior years was due to earnings manipulation. These years reflect a period in which financial reporting improprieties have ceased and have been disclosed. If firms over-invest during the manipulation period due to the distortion of information, then we expect firms to stop over-investing once the reported information is no longer distorted. Once capital markets and the board of directors realize the firm's true financial situation, they will not allow the over-investment to continue. After the manipulation period, we expect sample firms to invest in line with their revised fundamentals, or possibly less than fundamentals would indicate if the disclosure of improprieties results in a loss of access to capital required for investment.[5]

> **H2:** Firms that manipulate earnings will not invest at greater levels than expected given their investment opportunities in the post-manipulation period.

IV. RESEARCH DESIGN

Our study requires measures of two key constructs—earnings manipulation and excess investment. We use several approaches to measure each construct to provide confidence that our findings are not driven by measurement error in any one particular variable. This section discusses these two constructs and our measurement approach.

Identifying Earnings Manipulation

We employ two approaches to identify firms that misreport earnings. First, we use samples of firms that either (1) were accused of accounting improprieties by the Securities and Exchange Commission in enforcement actions (the SEC sample) or by shareholders in class action lawsuits (the litigation sample) or (2) admitted accounting misstatements by restating financial results (the GAO sample). Second, we use a measure of discretionary revenues to proxy for earnings manipulation.

Each approach to identifying earnings manipulation has advantages and disadvantages. Samples of firms that were accused of improprieties by the SEC or by shareholders could include firms falsely accused. Even if the allegations are true, the sample includes only those firms that were caught. This selection bias also applies to the GAO sample. Measures of discretionary accruals are commonly used to identify firms engaging in earnings management. This type of measure does not involve selection bias but may be subject to greater measurement error.

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[5] A decline in over-investment subsequent to the manipulation period is consistent with investment decisions makers relying on correct information or with reputation costs making it harder for sample firms to raise capital to finance investment. Dechow et al. (1996) find evidence of reputation costs—firms that received SEC enforcement actions experience significant increases in their costs of capital when the manipulations are made public. However, to the extent that the higher cost of capital is captured by our measure of investment opportunities, we control for this effect.

The SEC sample consists of 207 firms that are subject to accounting-related enforcement actions by the SEC during the period 1978–2002. The SEC takes enforcement actions against firms having allegedly violated the financial reporting requirements of securities laws, and publishes the details of these enforcement actions in a series of Accounting and Auditing Enforcement Releases (AAERs). We hand collect from the AAERs the name of the firms, the period over which the alleged violations occurred, and the nature of the allegations. We exclude firms with multiple enforcement actions and firms lacking the data required for the analyses.

The litigation sample includes 535 firms sued by shareholders for accounting improprieties during the period 1980–2002. The data source is the Class Action Securities Litigation Database provided by Woodruff-Sawyer & Co. It covers class action securities lawsuits since 1980, including virtually the entire population of federal shareholder lawsuits filed from 1988 to 2000. The sources for this database are the Securities Class Action Alert newsletters, the Securities Class Action Clearinghouse of Stanford Law School, press releases and wire service articles, the IPO Reporter newsletter, Moody's Corporation Data System, and various law firms and claims administration services. From the Woodruff-Sawyer database we obtain the name of the company sued, the beginning and ending dates of the class period, and the nature of the allegations.[6] We include only cases involving allegations of financial reporting improprieties. We also exclude firms subject to lawsuits over multiple litigation periods and firms lacking the data required for the analyses.

The GAO restatement sample consists of 313 firms that announced financial statement restatements due to accounting irregularities. The United States General Accounting Office issued a report on October 4, 2002, that listed restatements announced between January 1997 and June 2002. We obtain from the GAO database the name of the company and the nature of the restatement. We hand collect from SEC filings the fiscal period affected by the restatement. We exclude restatements that affect only interim periods. We also exclude firms with multiple restatements and firms lacking the data required for the analyses.

Of the SEC sample, the GAO sample, and the litigation sample, we focus primarily on the SEC sample for the following reasons. First, the GAO restatement sample contains many restatements (over 35 percent of the sample used in Srinivasan [2005]) that either are technical in nature or have a cumulative positive effect on earnings (i.e., the misstatement was income decreasing). We do not expect our predictions to hold for either of these types of restatements, which are included in our GAO sample. Second, although the litigation sample includes firms that were accused of earnings manipulation, even a costly settlement does not necessarily imply any wrongdoing by the firm (Alexander 1991).[7]

Several studies have shown that discretionary accrual models provide biased and low-powered estimates of discretion (for example, Dechow et al. 1995; Thomas and Zhang 2000;

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[6] We use the class period to identify the period in which the firm was manipulating its financial reporting. A class period is the period during which a company is alleged to have engaged in improper conduct, and is determined in part by company disclosures and the reaction of share prices. Because of the nature of financial reporting (i.e., financial results are announced after the fiscal period ends), the manipulation period usually starts before the beginning of the class period and ends before the conclusion of the class period (see discussion in Lu [2004]). Hence, we adjust the class period dates forward by three months to estimate the manipulation period dates.

[7] These three samples of firms reflect approaches widely used in the accounting literature to identify earnings management. To corroborate the occurrence of earnings management and its potential effects on investment decisions, we examined the predictive ability of our samples of earnings manipulators. In untabulated analyses, we find that in the years in which earnings are alleged to have been manipulated, earnings have lower predictive ability for future cash flows than in non-manipulation years. These findings support our primary argument that these firms engaged in earnings management and that this impaired the quality of their earnings information for investment decisions.

Dechow et al. 2003; Stubben 2008). To increase the power of our tests, we use a measure of discretionary revenues presented in Stubben (2008) to proxy for earnings manipulation in general. We estimate the following equation:

$$\Delta AR_{it} = \alpha + \beta \Delta S_{it} + \varepsilon_{it} \tag{1}$$

where ΔAR is the annual change in accounts receivable, S represents annual sales revenues, and Δ represents an annual change. Discretionary revenues are the residuals from these regressions, which are estimated separately for each industry-year group.[8] For discretionary revenues, our sample begins in 1988 because we use data from the statement of cash flows to calculate the change in accounts receivable.

Although measures of discretionary accruals are commonly used, measures of discretionary revenues have several advantages in our setting. First, Stubben (2008) shows that measures of discretionary revenues exhibit substantially less measurement error and bias than measures of discretionary accruals. Second, some accruals, such as depreciation, are closely related to investment and therefore cause a mechanical relation between capital investment and discretionary accruals. Third, discretionary revenues are closely tied to the investment decision, because growth in product demand is related to growth in revenues. Finally, manipulation of revenues is the most common form of earnings management. Nevertheless, untabulated results indicate identical inferences when using discretionary working capital accruals in place of discretionary revenues.

Identifying Excess Investment

We identify excess investment as investment that differs from the amount that would be predicted given the firm's investment opportunities, using a model motivated by the finance and economics literature on optimal investment. In addition, we examine the sensitivity of our findings to three alternative estimates of expected investment: (1) the median investment in the firm's industry in the same year, (2) the investment of a control firm matched on industry, year, size, and asset growth, and (3) the investment of a control firm matched on industry, year, size, and a different measure of excess investment the year before the manipulation.

A large literature in finance and economics studies investment decisions, and attempts to understand the factors that influence investment behavior, and how changes in monetary policy or other policies affect investment.[9] Empirical analyses in this literature have generally employed the following model of investment:

$$INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 CF_{it} + \varepsilon_{it} \tag{2}$$

where INV_{it} is the investment level for firm i in year t, $Q_{i,t-1}$ is the beginning of year t market value of assets divided by book value of assets, and CF_{it} is a measure of firm-level cash flows.

The linear relation between investment and $Q_{i,t-1}$ is motivated by models of investment that incorporate adjustment costs and linear homogeneity in the production function. Modigliani and Miller (1958) show that in perfect capital markets, investment depends only on investment opportunities, and Tobin (1969) shows that investment opportunities are summarized in marginal q. Hayashi (1982) provides conditions under which marginal q is equivalent to average Q, which leads to the commonly used formulation above. CF_{it} is included to control for differences in internal financing capability. However, a number of studies, including Poterba (1988), Kaplan and

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[8] Industries are as defined in Barth et al. (2005). See descriptions in Table 2.

[9] Hubbard (1998) provides an overview of this literature.

Zingales (1997), and Alti (2003), have questioned whether its coefficient captures differences in financing constraints, suggesting that differences in its coefficient may also reflect measurement error in Q.

We also estimate a modified version of Equation (2). This version controls for asset growth, past investment, and allows for variation in the relationship between investment and Tobin's Q. Because of the possibility that growth firms are more likely to invest independent of any misstatements and to address potential measurement error in Tobin's Q, we include asset growth at the beginning of the year. In addition, we control for investment in the prior year. This serves two main purposes. First, it captures a firm-specific component to investment decisions not captured by the other variables in the model. Second, it adds a change component to the model. Residual investment is measured incremental to the persistent portion of the prior year's investment.[10]

We estimate the investment model separately for each industry and year. This approach implicitly assumes that the responsiveness of investment to investment opportunities is constant across firms in the same industry-year. However, Abel and Eberly (2002) show that adjustment costs are not linear and thus the relationship between investment and Tobin's Q is a function of Tobin's Q. Therefore, we also augment Equation (2) to include incremental coefficients for the quartiles of Tobin's Q.

$$\begin{aligned} INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 Q\_QRT2_{i,t-1} + \beta_3 Q\_QRT3_{i,t-1} + \beta_4 Q\_QRT4_{i,t-1} + \beta_5 CF_{it} \\ + \beta_6 GROWTH_{i,t-1} + \beta_7 INV_{i,t-1} + \varepsilon_{it} \end{aligned} \tag{3}$$

where $GROWTH_{i,t-1}$ equals the natural log of total assets at the end of year $t-1$ divided by total assets at the end of year $t-2$. $Q\_QRT2_{i,t-1}$ ($Q\_QRT3_{i,t-1}$, $Q\_QRT4_{i,t-1}$) equals $Q_{i,t-1}$ times an indicator variable that equals 1 if $Q_{i,t-1}$ is in the second (third, fourth) quartile of its industry-year distribution. We also allow the intercept, α, to vary across the quartiles of $Q_{i,t-1}$.[11]

Our tests assume that the ability of $Q_{i,t-1}$ and CF_{it} to proxy for the firm's opportunity set does not vary through time. Beginning book value of equity and market value of equity should be unaffected by the manipulation in the first year of the misreporting period. Therefore, our findings in the first year of the misreporting period are likely unbiased, as the firm's true investment opportunities should be captured by $Q_{i,t-1}$. During the second and third years of the misstatement, it is likely that $Q_{i,t-1}$ is overstated, which would bias against finding over-investment.[12] In the first year after the misreporting period, the beginning market value of equity may not reflect the firm's true future prospects, and book value of equity is potentially overstated due to the financial reporting manipulation. This could cause $Q_{i,t-1}$ to be either too low or too high in this year, though we expect that in most cases, $Q_{i,t-1}$ is overstated.[13] However, in the second and third years after the misstatement, $Q_{i,t-1}$ should be unaffected. Due to these potential limitations, we use three additional measures of excess investment.

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[10] In untabulated sensitivity analyses, in place of lagged investment we use lagged residual investment based on Equation (2); our inferences are unchanged.

[11] We also estimate a second modified version of Equation (2) that aims to mitigate the issue of selection bias in our samples. We also include cash flows one and two years into the future to address the possibility that poor subsequent performance is a factor related to these firms appearing in our samples. Untabulated results reveal that our inferences are unchanged.

[12] On average we observe an increase in investment opportunities through time, consistent with overstatement in the misreporting period.

[13] If the multiple on misstated earnings exceeds the market to book ratio absent manipulation, then $Q_{i,t-1}$ will be overstated at the start of the year that earnings manipulation is identified and corrected.

The first alternative measure of excess investment is the firm's investment less the median investment for the same industry and year. The second alternative measure of excess investment controls for industry, year, size, and the rate of asset growth. Specifically, for each sample firm, in the year before the manipulation period, we choose a control firm with the closest asset growth (*GROWTH*) among firms in the same industry, year, and size group. Firms in the same size group have a natural log of total assets (*TA*) within $2 million of the sample firm. If there are ties in asset growth, then we choose the firm with the closest size. Growth-adjusted investment is investment less the control firm's investment. The third alternative measure of excess investment controls for the possibility that some firms might choose to continuously over-invest independent of any misstatements. This measure of excess investment controls for industry, year, size, and a different measure of excess investment. We follow the same procedure as above, except that we choose the firm from the same industry, year, and size group with the closest excess investment in the prior year measured as the residual from Equation (2). Adjusting for past excess investment controls for the possibility that some firms are generally more ambitious than others with respect to investment.

Using control samples has several advantages over the investment model. First, we do not have to assume the validity of a particular model. Second, control samples are not affected by measurement error in model inputs through the manipulation period. Finally, control samples allow us to address the over-investment we observe in the years leading up to the manipulation period when using Equation (2). We match sample firms to control firms with similar investment patterns in the years prior to the manipulation but didn't manipulate earnings. This comparison examines whether sample firms invest more than they would have had they not manipulated earnings.

Data

The financial statement and market value data used in this study are obtained from the Compustat annual file. Our data spans the years 1975 to 2005 to cover the sample period of 1978 to 2002 plus three years leading up to and following the alleged misreporting periods. To calculate ratios, we require net property, plant, and equipment (*NPPE*, item #8) to be greater than zero. Capital expenditures (*INV*) are taken from the statement of cash flows when available (item #128, otherwise item #30).[14] Our proxy for Tobin's Q is $(MVE + TA - BVE) / TA$, where *MVE* is the market value of equity (item #25 × item #199) and *BVE* is the book value of common equity (item #60), both measured at the beginning of the year.[15] Cash flows (*CF*) are taken from the statement of cash flows when available (item #308); otherwise the balance sheet approach is used.[16] In this case, $CF = OIAD - (\Delta CA - \Delta Cash) - (\Delta CL - \Delta STD - \Delta TP) - DEPR$, where *OIAD* is operating income after depreciation (item #178), *CA* is current assets (item #4), *Cash* is cash and cash equivalents (item #1), *CL* is current liabilities (item #5), *STD* is debt included in current liabilities (item #34), *TP* is income taxes payable (item #71), and Δ is the first-difference operator.

We obtain measures of external financing using data from the statement of cash flows. We sum cash proceeds from the issuance of debt (item #111) and from the sale of common and preferred stock (item #108). We determine the change in accounts receivable also from the statement of cash flows (item #302). We measure revenues from the income statement (item #12), and each of the variables used in the calculation of discretionary revenues is deflated by average total assets (item #6). Non-missing values for each of the variables described above are required for each firm-year observation to be included in the sample. The final sample includes 134,561 observations for 16,023 unique firms.

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[14] Richardson (2006) uses a measure of investment that includes acquisitions and research and development (R&D). We find similar but slightly weaker (but still statistically significant) patterns of over-investment using this measure. However, we prefer focusing on capital expenditures. R&D usually has an immediate earnings impact, so firms managing earnings might be inclined to under-invest, rather than over-invest, in R&D. In addition, acquisitions are often larger and less frequent than capital expenditures, making it more difficult to observe trends through time.

[15] A more precise proxy for Tobin's Q requires that the book value of assets be adjusted to reflect replacement costs. Perfect and Wiles (1994) suggest that this adjustment is not critical.

[16] Although a commonly used proxy for cash flows in this stream of literature is net income plus depreciation, Bushman et al. (2005) show it can be problematic.

Empirical Procedures

We use the prediction error from Equations (2) and (3), which are estimated after excluding manipulation sample firms, as a measure of excess investment for sample firm *i* in year *t*. Our approach to examining the influence of manipulated information on investment decisions is to examine the behavior of excess investment through time relative to the misreporting period. We examine the mean level of excess investment over the three years preceding the misreporting period, the year or years during the misreporting period, and the three years following the misreporting period. Our estimation assumes that the proxies for the investment opportunity set capture the firm's optimal investment at each point in time, and therefore excess investment that is significantly different from zero reflects deviations from optimal investment for the misreporting sample. We predict significantly positive excess investment during the misreporting period.

Our main results use a regression approach to adjust for the expected level of investment given the firm's investment opportunities. However, ordinary regression approaches can be sensitive to large outliers, which are particularly troublesome when ratios are used. We use rank regressions to protect the results from the effects of outliers.[17] Rank regressions use independently ranked values, from lowest to highest, for each of the regression variables, which are then scaled between 0 and 1. Our tests that do not involve regressions use median values to mitigate the effects of outliers. When testing median investment levels, we use a signed-rank test for significance of excess investment. Otherwise, we use a t-test.

One competing explanation for finding a relationship between earnings management and investment is that managers with profitable investment projects who face financing constraints manipulate earnings in order to obtain less expensive external financing, which they then use to invest. If this story alone explains the over-investment, then we would expect to see *ex post* over-investment only for those firms that relied on external financing. We would not expect to see over-investment by firms that did not obtain external financing. We test this competing explanation by examining separately the investment behavior of firms with low and high levels of external financing. Firms are considered to have a low (high) level of external financing if the combined cash proceeds from debt and equity issuances is less than (at least) 25 percent of capital expenditures for the year.[18]

It is also possible that over-investment leads to earnings manipulation. That is, firms that over-invest are more likely to manage earnings subsequently to cover up lower returns on suboptimal investment. We use our measures of discretionary revenues to provide additional evidence on the timing of earnings manipulation and excess investment. First, we augment Equations (2) and (3) with measures of discretionary revenues leading up to, concurrent with, and following the investment decision.

$$INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 CF_{it} + \beta_3 DREV_{i,t-2} + \beta_4 DREV_{i,t-1} + \beta_5 DREV_{it} + \beta_6 DREV_{i,t+1} + \beta_7 DREV_{i,t+2} + \varepsilon_{it}. \tag{4}$$

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[17] Winsorized regressions produce qualitatively similar results.

[18] We obtain qualitatively similar results when using 50 percent of capital expenditures. Because most firms obtain at least some amount of financing, we are unable to reliably split on whether or not the firm obtained any external financing.

$$INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 Q\_QRT2_{i,t-1} + \beta_3 Q\_QRT3_{i,t-1} + \beta_4 Q\_QRT4_{i,t-1} + \beta_5 CF_{it} \\ + \beta_6 GROWTH_{i,t-1} + \beta_7 INV_{i,t-1} + \beta_8 DREV_{i,t-2} + \beta_9 DREV_{i,t-1} + \beta_{10} DREV_{it} \\ + \beta_{11} DREV_{i,t+1} + \beta_{12} DREV_{i,t+2} + \varepsilon_{it}. \quad (5)$$

In addition, we conduct Granger causality tests to determine whether excess investment or earnings management leads the other. Granger (1969) proposes a test for causality that involves conducting F-tests to see whether lagged information of one variable (X) provides any statistically significant information about a second variable (Y) in the presence of lagged values of that second variable. If so, then X Granger causes Y. We conduct Granger causality tests using one and two lags of each variable.

V. RESULTS

Descriptive Statistics

Descriptive statistics for the three main samples are presented in Table 1. Table 1, Panel A presents the distribution of cases of manipulation by year. The GAO restatements do not begin until the 1990s, which is a consequence of the database covering only the more recent restatement announcements. In each sample, the number of cases exhibits an increasing trend over time that trails off over the last few years of the sample. The trailing off could reflect that cases beginning in the early 2000s are still being discovered.

Table 1, Panel B presents the distribution of sample manipulations across industries. In each sample, a disproportionately large number of cases occur in the Computers industry, which includes computer hardware, software, and electronic components firms. Of the firms in the SEC (GAO, litigation) sample, 23 percent (20 percent, 26 percent) are in the Computers industry, whereas only 12.5 percent of the total sample is in this industry. The Services industry is next with 9 percent (14 percent, 11 percent) of the cases for the SEC (GAO, litigation) sample. In general, the cases occur in every industry, with most industries having several cases.

Table 2 presents summary statistics for the entire sample. Descriptive statistics are presented in Panel A, and correlations are presented in Panel B. The large differences between mean and median statistics, and the large standard deviations indicate the influence of outliers on the mean and standard deviation of many of the variables. For this reason, we focus our discussion on the medians and quartiles. At the median, firms invest 21 percent of net property, plant, and equipment (*NPPE*). The inter-quartile ranges indicate significant cross-sectional variation in these amounts. Specifically, at the quartiles, investment ranges from 11 percent to 40 percent of *NPPE*. The median Q is 1.32, consistent with unrecognized assets causing the market value of assets to exceed the book value of assets. Median cash flow from operations is positive (19 percent of *NPPE*), but cash flows at the first quartile are negative (−4 percent of *NPPE*). The median asset growth is 0.08, with an inter-quartile range between −0.03 and 0.22. The mean excess investment (*XINV*) is zero by construction, but the median is negative (−0.07), indicating potential skewness in the data. Mean discretionary revenues (*DREV*) are zero by construction, and the median is also zero. Because the measure of discretionary revenues relies on information contained in the statement of cash flows, which is generally first available in 1988, the sample size for this measure is less than that of the total sample.

Due to the influence of outliers noted above, Spearman (rank) correlations are tabulated in Panel B of Table 2. All correlations are significantly different from zero.[19] Investment is positively

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[19] We use the term "significant" or "statistically significant" to denote statistical significance at less than the 0.05 level based on a one-sided test when we have signed predictions and a two-sided test otherwise.

TABLE 1
Earnings Manipulation Sample Composition

Panel A: Distribution by Year

	SEC Sample		GAO Sample		Litigation Sample	
Year	**Count**	**Median Length**	**Count**	**Median Length**	**Count**	**Median Length**
1978	4	3				
1979	1	6				
1980	4	4			1	6
1981	5	3				
1982	9	2			1	2
1983	7	1				
1984	4	2				
1985	6	2.5			2	2.5
1986	6	2			2	3
1987	10	2			6	2.5
1988	6	3.5			11	3
1989	15	2			13	2
1990	8	1.5			8	2
1991	21	2			14	3
1992	16	2	1	6	13	2
1993	8	2			27	2
1994	11	2	6	2.5	26	2.5
1995	10	2.5	15	3	27	2
1996	14	2	33	2	44	2
1997	10	2.5	39	2	55	2
1998	11	2	57	2	60	2
1999	4	2.5	48	2	66	2
2000	5	1	65	1	61	2
2001	7	1	20	1	59	2
2002	1	1	1	1	16	1
Total	203		285		512	

(continued on next page)

TABLE 1 (continued)

Panel B: Distribution by Industry

	SEC Sample		GAO Sample		Litigation Sample	
Industry	**Count**	**Median Length**	**Count**	**Median Length**	**Count**	**Median Length**
Mining and Construction	5	1	5	3	11	2
Food	7	4	8	1.5	13	2
Textiles, Printing, and Publishing	15	2	16	2	22	2
Chemicals	5	2	6	1.5	8	2
Pharmaceuticals	7	1	8	1.5	15	2
Extractive	3	2	7	1	6	2.5
Manufacturers:						
Rubber and Glass	1	1	5	1	5	2
Metals	8	2	9	2	10	3
Machinery	5	3	13	1	9	2
Electrical Equipment	15	2	13	2	29	2
Transportation Equipment	4	3.5	2	2.5	10	2
Instruments	10	2	21	2	32	2
Miscellaneous	2	2.5	2	1.5	3	2
Computers	47	2	57	2	132	2
Transportation	6	2	8	2	20	2
Utilities	8	3.5	10	1	23	3
Retail:						
Wholesalers	11	3	13	2	26	3
Miscellaneous	17	2	26	2	39	2
Restaurants			2	2	6	2
Financial Services	6	2	10	2	30	2
Insurance and Real Estate	3	1	3	1	7	3
Services	18	2	41	2	56	2
Total	203		285		512	

Count is the number of SEC enforcement actions, restatements, or litigation cases. Median length is the median number of years of the misreporting period. Industries are defined in Barth et al. (2005).

correlated with Q; firms with greater growth opportunities tend to invest more. Investment is also positively correlated with profitability, as measured by cash flows, and with asset growth. To

TABLE 2
Sample Summary Statistics

Panel A: Descriptive Statistics

Variable	n	Mean	Std. Dev.	Q1	Median	Q3
INV	134,561	0.47	8.37	0.11	0.21	0.40
Q	134,561	2.19	6.05	1.01	1.32	2.07
CF	134,561	0.17	206.47	−0.04	0.19	0.51
GROWTH	134,561	0.13	0.44	−0.03	0.08	0.22
XINV	134,561	0.00	0.39	−0.18	−0.07	0.06
DREV	87,773	0.00	0.05	−0.02	0.00	0.02

Panel B: Spearman Correlation Matrix

	INV	*Q*	*CF*	*GROWTH*	*XINV*
Q	0.33				
CF	0.15	0.01			
GROWTH	0.39	0.25	0.09		
XINV	0.73	−0.03	0.13	0.25	
DREV	0.06	0.05	−0.13	−0.01	0.06

Variable Definitions:

INV = capital expenditures scaled by beginning-of-year net property, plant, and equipment;
Q = Tobin's Q (market to book value of assets) at beginning of year;
CF = cash flow from operations scaled by beginning-of-year property, plant, and equipment;
GROWTH = natural log of total assets at end of prior year divided by total assets two years prior;
XINV = excess investment, measured as the residual from an industry-year regression of *INV* onto *Q* and *CF*; and
DREV = discretionary revenues, measured as the residual from an industry-year regression of the change in accounts receivable onto the change in revenues, each scaled by average total assets.

All variables are as originally reported. All correlations in Panel B are significantly different from zero ($p < 0.05$). Sample period ranges from 1975–2005.

control for additional factors influencing investment, we base our inferences on the industry-adjusted, growth-adjusted, investment-adjusted, and multivariate relations presented in Tables 3–6.

Table 3 presents median statistics through event time. Median levels of investment for the three samples are presented in Panel A, and plotted in Figure 1. Statistics on investment opportunities are presented in Panel B, and statistics on cash flows from operations are presented in Panel C.

The SEC sample results in Panel A of Table 3 and Figure 1 reveal a slight upward trend in investment during the three years leading up to the manipulation period. Investment begins at 26 percent of net property, plant, and equipment (*NPPE*) and increases to 30 percent. Investment then increases dramatically and peaks in the first year of the manipulation period—39 percent of *NPPE*—and declines thereafter. By three years after the manipulation period median investment

TABLE 3
Median Statistics through Event Time

Panel A: Investment

	SEC Sample			GAO Sample			Litigation Sample		
Event Year	**n**	***INV***	**Ind. Adj.**	**n**	***INV***	**Ind. Adj.**	**n**	***INV***	**Ind. Adj.**
−3	129	0.26	**0.03**	212	0.27	**0.04**	336	0.30	**0.05**
−2	144	0.27	**0.04**	236	0.28	**0.04**	391	0.33	**0.08**
−1	165	0.30	**0.10**	253	0.31	**0.04**	457	0.39	**0.12**
M1	203	0.39	**0.11**	285	0.31	**0.07**	512	0.44	**0.16**
M2	149	0.37	**0.11**	171	0.30	**0.02**	459	0.39	**0.14**
M3	77	0.25	**0.02**	61	0.23	0.00	190	0.31	**0.06**
1	182	0.20	−0.04	275	0.19	−0.01	532	0.20	−0.03
2	175	0.18	**−0.05**	258	0.20	0.00	484	0.18	**−0.03**
3	168	0.19	−0.05	235	0.18	0.00	445	0.19	−0.03

Panel B: Investment Opportunities

	SEC Sample			GAO Sample			Litigation Sample		
Event Year	**n**	***Q***	**Ind. Adj.**	**n**	***Q***	**Ind. Adj.**	**n**	***Q***	**Ind. Adj.**
−3	129	1.50	**0.12**	212	1.64	**0.12**	336	1.71	**0.22**
−2	144	1.49	**0.07**	236	1.71	**0.19**	391	1.78	**0.33**
−1	165	1.58	**0.18**	253	1.60	**0.11**	457	1.84	**0.35**
M1	203	1.82	**0.27**	285	1.66	**0.11**	512	2.02	**0.52**
M2	149	1.73	**0.21**	171	1.66	**0.04**	459	1.94	**0.44**
M3	77	1.96	**0.32**	61	1.44	−0.03	190	1.75	**0.22**
1	182	1.57	**0.14**	275	1.36	−0.08	532	1.37	−0.11
2	175	1.47	0.02	258	1.34	−0.09	484	1.38	−0.12
3	168	1.40	0.00	235	1.32	−0.16	445	1.39	−0.11

(continued on next page)

TABLE 4 (continued)

Panel C: Excess Investment for GAO Restatement Sample Firms

		(2)		(3)	
Event Year	**n**	**Mean *XINV***	**t-stat**	**Mean *XINV***	**t-stat**
−3	215	0.04	2.31	0.02	1.82
−2	242	0.04	2.10	0.00	0.35
−1	256	0.04	2.81	0.02	1.60
M1	288	0.06	3.81	0.03	2.31
M2	173	0.05	2.43	0.02	1.02
M3	61	0.01	0.44	−0.01	−0.25
1	278	0.00	−0.15	−0.02	−1.48
2	261	0.00	0.10	0.02	1.51
3	239	0.00	0.17	0.00	0.03

Panel D: Excess Investment for Litigation Sample Firms

		(2)		(3)	
Event Year	**n**	**Mean *XINV***	**t-stat**	**Mean *XINV***	**t-stat**
−3	338	0.04	3.23	0.02	1.95
−2	393	0.06	4.83	0.02	2.37
−1	459	0.08	6.64	0.04	4.65
M1	515	0.12	11.06	0.06	7.15
M2	463	0.11	8.91	0.03	3.01
M3	191	0.06	3.44	0.00	−0.07
1	535	−0.03	−2.82	−0.07	−7.70
2	490	−0.04	−3.33	0.00	0.17
3	450	−0.04	−3.05	0.01	0.67

Panel A presents summary statistics from industry-year regressions of investment. Mean Estimate is the mean of 616 separate coefficient estimates, and FM t-stat is the Fama-MacBeth t-statistic. M1 (M2, M3) represents the first (second, third) year of the misreporting period. The main and incremental intercepts are not tabulated.

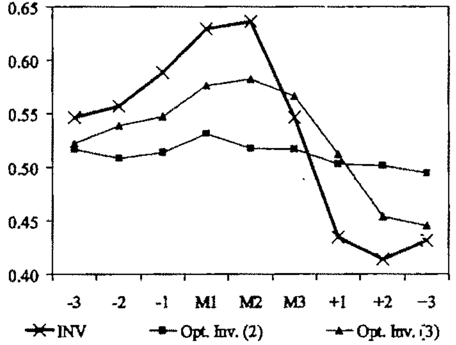
Panels B through D present the mean of excess investment (*XINV*) through event time for sample firms, based on Equations (2) and (3). *Q_QRT2* (*Q_QRT3*, *Q_QRT4*) equals *Q* times an indicator variable that equals 1 if *Q* is in the second (third, fourth) quartile of its industry-year distribution.

All other variables are defined in Table 2, and each is ranked and scaled between 0 and 1 (by industry and year) before estimating the models.

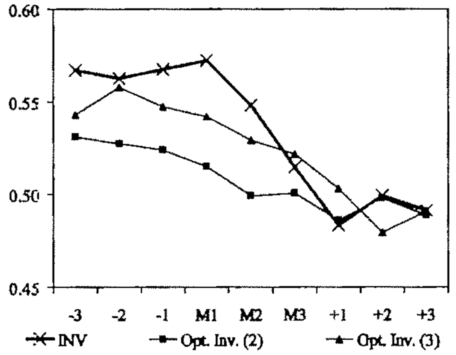
cases we find evidence of significant over-investment during the manipulation period, and that over-investment ends as the manipulation ends. In Panel A, we find similar magnitudes of over-investment for SEC firms in the first year of the manipulation period (0.09, t = 2.00 for firms with low financing; 0.11, t = 4.87 for firms with high financing) and the second year of the manipulation period (0.10, t = 2.05 for firms with low financing; 0.13, t = 5.21 for firms with high financing). It is interesting to note that significant over-investment before the manipulation period occurs only in

FIGURE 2
Plots of Investment and Optimal Investment According to Q-Model[a]

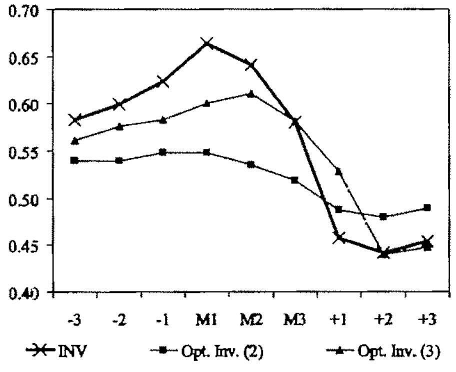
Panel A: SEC Sample Firms



Panel B: GAO Sample Firms



Panel C: Litigation Sample Firms



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[a] These charts present the mean investment (*INV*, which equals capital expenditures scaled by net property, plant, and equipment at the beginning of the year) through event time for firms in the SEC enforcement action sample (Panel A), the GAO restatement sample (Panel B), and the Woodruff-Sawyer securities litigation sample (Panel C). Also presented are two measures of optimal investment that are based on Equations (2) and (3). "Opt. Inv. (2)" ("Opt. Inv. (3)") is the mean of each sample firm's predicted value from Equation (2) (Equation (3)). Each variable is ranked and scaled between 0 and 1 (by industry and year) before estimating the equations. M1 (M2, M3) represents the first (second, third) year of the misreporting period.

TABLE 5

Excess Investment through Event Time—By Level of External Financing

$$INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 CF_{it} + \varepsilon_{it} \tag{2}$$

Panel A: Excess Investment for SEC Sample Firms

	Low Financing			High Financing		
Event Year	**n**	**Mean *XINV***	**t-stat**	**n**	**Mean *XINV***	**t-stat**
−3	40	0.00	−0.02	81	0.05	1.55
−2	31	0.06	1.16	104	0.05	1.92
−1	39	0.01	0.25	118	0.10	3.69
M1	41	0.09	2.00	153	0.11	4.87
M2	26	0.10	2.05	115	0.13	5.21
M3	21	0.00	−0.01	54	0.04	0.88
1	58	−0.06	−1.48	114	−0.08	−2.80
2	49	−0.06	−1.42	115	−0.09	−4.03
3	49	−0.06	−1.40	108	−0.06	−2.51

Panel B: Excess Investment for GAO Restatement Sample Firms

	Low Financing			High Financing		
Event Year	**n**	**Mean *XINV***	**t-stat**	**n**	**Mean *XINV***	**t-stat**
−3	68	0.05	2.03	134	0.04	1.72
−2	59	0.01	0.23	163	0.05	2.26
−1	56	0.00	0.14	183	0.06	3.17
M1	57	0.03	0.86	211	0.07	4.04
M2	25	0.20	4.60	130	0.03	1.29
M3	19	−0.05	−0.81	38	0.06	1.61
1	65	0.01	0.38	192	0.00	−0.25
2	54	0.04	1.04	185	−0.01	−0.26
3	59	0.04	1.22	158	0.00	0.06

(continued on next page)

TABLE 5 (continued)

Panel C: Excess Investment for Litigation Sample Firms

	Low Financing			High Financing		
Event Year	**n**	**Mean *XINV***	**t-stat**	**n**	**Mean *XINV***	**t-stat**
−3	70	0.01	0.38	243	0.06	3.70
−2	76	0.07	2.46	297	0.06	3.91
−1	59	0.07	2.40	367	0.08	6.02
M1	53	0.15	5.65	427	0.11	9.47
M2	74	0.12	4.62	367	0.11	7.91
M3	34	0.06	1.51	142	0.07	3.26
1	156	−0.01	−0.66	343	−0.04	−2.86
2	127	−0.03	−1.10	327	−0.04	−2.72
3	120	−0.02	−0.88	298	−0.04	−2.63

M1 (M2, M3) represents the first (second, third) year of the misreporting period. Panels A through C present the mean of excess investment (*XINV*) through event time for sample firms, based on Equation (2).

Variables are defined in Table 2, and each is ranked and scaled between 0 and 1 (by industry and year) before estimating the models. Firms are considered to have obtained low (high) levels of external financing if the combined cash proceeds from debt and equity issuances are less than (at least) 25 percent of capital expenditures for the year.

the sample of firms with a high level of external financing. Thus, it is possible these firms managed earnings to obtain financing, or to obtain cheaper financing. However, finding over-investment for the sample of firms with low amounts of external financing supports H1. Similar inferences are obtained from the GAO and litigation samples in Panels B and C, except in Panel C we see over-investment before the manipulation period for both sets of firms.

Table 6 presents the investment of sample firms relative to that of control firms based on past growth and excess investment. Comparing investment of sample firms to that of control firms matched on asset growth the year before the manipulation period, we no longer find evidence of over-investment leading up to the manipulation. Although sample firms invest significantly more than the industry median in the pre-manipulation period, their investment is not significantly greater than that of firms with similar growth. These findings indicate that prior to the misreporting period, greater investment is likely due to high growth expectations. However, growth cannot explain the even greater levels of investment in the first two years of the misreporting period. SEC sample firms invest significantly more than matched firms during the first two years of the manipulation period (7 percent of *NPPE*), which is consistent with H1. Furthermore, SEC sample firms invest less than growth-matched firms after the manipulation period (1 percent, 3 percent, and 0 percent of *NPPE*); these differences are not statistically significant.

We obtain similar results when controlling for excess investment the year before the manipulation period. We do not find evidence of over-investment leading up to the manipulation period after controlling for excess investment in event year $t-1$. However, SEC sample firms invest more than control firms during the first two years of the manipulation period (7 percent and 6 percent of *NPPE*), which is consistent with H1. Investment of sample firms is then significantly lower than that of control firms the first year after the manipulation. This evidence is consistent with both

TABLE 6

Median Investment through Event Time—Relative to Control Firms

	SEC Sample				GAO Sample				Litigation Sample			
Event Year	**n**	***INV***	**Grw. Adj.**	**Inv. Adj.**	**n**	***INV***	**Grw. Adj.**	**Inv. Adj.**	**n**	***INV***	**Grw. Adj.**	**Inv. Adj.**
−3	129	0.26	0.00	−0.01	212	0.27	0.01	0.04	336	0.30	0.01	0.01
−2	144	0.27	0.01	0.00	236	0.28	−0.01	0.03	391	0.33	**0.02**	0.02
−1	165	0.30	0.02	0.00	253	0.31	−0.01	0.01	457	0.39	**0.04**	**0.01**
M1	203	0.39	**0.07**	**0.07**	285	0.31	**0.04**	**0.04**	512	0.44	**0.09**	**0.07**
M2	149	0.37	**0.07**	**0.06**	171	0.30	0.02	**0.04**	459	0.39	**0.04**	**0.06**
M3	77	0.25	0.00	0.03	61	0.23	0.00	0.01	190	0.31	0.02	**0.02**
1	182	0.20	−0.01	**−0.04**	275	0.19	0.01	−0.01	532	0.20	**−0.03**	**−0.04**
2	175	0.18	−0.03	−0.02	258	0.20	0.00	−0.02	484	0.18	−0.01	**−0.03**
3	168	0.19	0.00	0.00	235	0.18	0.01	−0.01	445	0.19	0.00	−0.01

Adjusted values significantly different from zero at the 5 percent level are shown in bold.

Table 6 presents the median investment (*INV*, which equals capital expenditures scaled by net beginning-of-year property, plant, and equipment) through event time for firms in the SEC enforcement action sample, the GAO restatement sample, and the Woodruff-Sawyer securities litigation sample. Also presented are two adjusted measures of investment. Grw. Adj. (Inv. Adj.) is the median investment of each sample firm's investment less that of a control firm matched on asset growth (excess investment), size, and industry, in event year $t-1$. Missing data for control firms reduces the effective sample sizes for adjusted investment. M1 (M2, M3) represents the first (second, third) year of the misreporting period.

sample and control firms over-investing relative to the industry median before the manipulation period, but while the investment of control firms immediately begins to revert to the industry median, that of sample firms continues to increase through the manipulation period until the manipulation is uncovered.

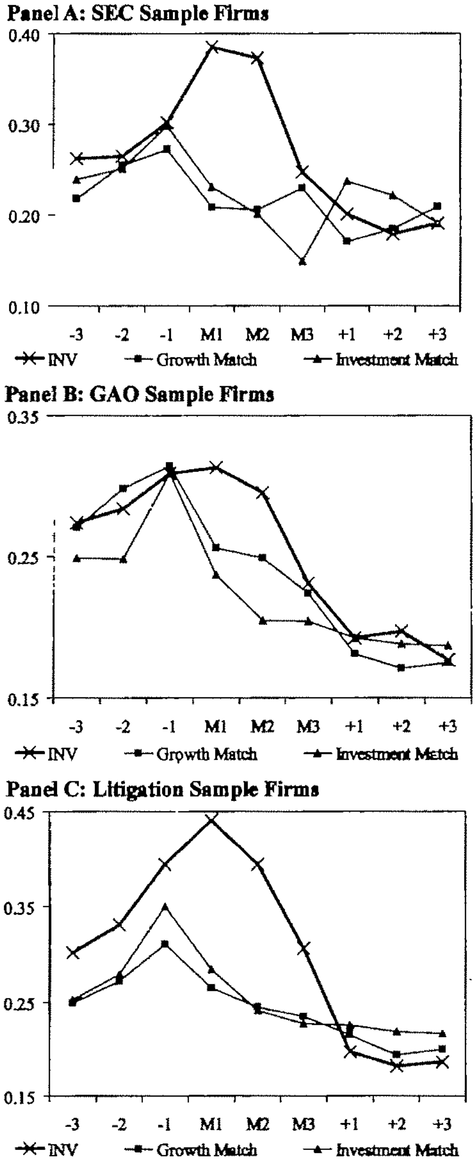
The results using the GAO sample are similar to those using the SEC sample. In both cases, we find no evidence of over-investment before the manipulation period; however, sample firms invest more than control firms during each of the three years of the manipulation period. Consistent with H1, this difference is significant in the first year for both sets of control firms and significant in the second year for the investment control sample. In both cases, we find no evidence of over-investment subsequent to the manipulation.

The litigation sample results in Table 6 indicate that sample firms invest slightly more than control firms leading up to the manipulation period, and they generally invest even more than control firms during the manipulation period. Consistent with H1, litigation sample firms invest an additional 9 percent, 4 percent, and 2 percent of *NPPE* more than control firms based on growth, and 7 percent, 6 percent, and 2 percent of *NPPE* more than control firms based on past excess investment during the three years of the manipulation period. With both sets of control firms, investment of sample firms is no longer greater than that of control firms after the manipulation period. Median investment for sample and control firms is plotted in Figure 3.

Alternative Explanations for the Results

In the previous section, we find a positive association between earnings management and excess investment, which we claim supports our hypothesis that earnings manipulation contributes

FIGURE 3
Plots of Investment for Sample and Control Firms[a]


Panel A: SEC Sample Firms

Panel B: GAO Sample Firms

Panel C: Litigation Sample Firms

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[a] These charts present the median investment (*INV*, which equals capital expenditures scaled by net property, plant, and equipment at the beginning of the year) through event time for firms in the SEC enforcement action sample (Panel A), the GAO restatement sample (Panel B), and the Woodruff-Sawyer securities litigation sample (Panel C). Also presented are two control measures of investment. "Growth Match" ("Investment Match") is the median investment of each sample firm's control firm matched on asset growth (excess investment), size, and industry, in event year $t-1$. M1 (M2, M3) represents the first (second, third) year of the misreporting period.

to suboptimal investment decisions by distorting information relevant to the investment decision. However, there are several alternative explanations for the documented relation.

One alternative explanation is that firms manipulate earnings to obtain external financing for potential investment projects. Although external financing is a plausible motivation for earnings management, this story does not imply that firms would over-invest with the funds obtained, as ours does. Nevertheless, we find evidence of over-investment for firms with both low and high levels of external financing, which is consistent with our hypothesis that distorted financial information played a role in the over-investment.

A second potential explanation is that growth firms are more likely to manage earnings and our measure of optimal investment does not fully capture the implications of growth. A key feature of our design is the inclusion of specific controls for growth. The tests based on Equation (3), which are reported in Table 4, control for growth. The tests reported in Table 6 match our sample firms on growth in year $t-1$, as well as on industry, size, and year. Our findings document that relative to other firms with comparable asset growth, sample firms that misreported their financial statements over-invest during the misreporting period.

A third alternative explanation for the positive relation between earnings manipulation and excess investment is error in our measure of excess investment. To provide confidence that our results are not driven by measurement error, we use several measures of excess investment. We measure excess investment using the Q model of investment and the Q model augmented with asset growth and past investment. Because Q is potentially misstated during the second and third years of the manipulation and the first year after the manipulation, we present industry- and control-firm-adjusted results in Tables 3 and 6. These results provide confirmation that the results presented in Table 4 are not driven by measurement error in Q.

A fourth explanation is that firms are manipulating the capitalization of expenses as a way to manage earnings. This explanation suggests the measure we use for capital expenditures does not represent real investment outlays. We address this concern with three untabulated analyses. First, we use restated capital expenditures from Compustat, which reflects revised rather than manipulated capital expenditures. Second, we use data on the type of financial reporting issue giving rise to the restatement or enforcement action to exclude firms that were accused of or admitted to improperly capitalizing expenses. Third, we exclude firms in industries where improper capitalization was most prevalent (technology and telecommunications). In each case, the tenor of the results was unchanged.

A fifth possibility is that firms may manage earnings to cover up poor returns on past over-investment. That is, rather than earnings manipulation leading to over-investment, past over-investment leads to earnings manipulation. We find this explanation less compelling for two reasons. First, over-investment leading to earnings manipulation would predict the greatest over-investment prior to the manipulation whereas we find the greatest over-investment concurrent with the manipulation. Second, we find that over-investment continues beyond the first year of the manipulation period, and it is not clear why management would continue over-investing when it is already facing low returns on past over-investment. Nevertheless, we use measures of discretionary revenues to provide additional evidence on the timing of earnings manipulation and excess investment.

Finally, despite our efforts to control for specific factors that may be related to both earnings management and the level of investment, our results may be affected by some other (unknown) omitted factor. We attempt to control for the effects of omitted variables by examining changes in investment through event time. This is partially accomplished in Equation (3), which includes lagged investment as an explanatory variable. In addition, we estimate a changes specification of Equation (2). Untabulated results reveal that sample firms significantly increase investment during the first year of the manipulation period ($t = 2.55$, 3.45, and 9.00 for the SEC, GAO, and litigation samples, respectively). After the first year, over-investment continues but increases at a lower rate (t = 0.52, 0.59, and 3.12 for the SEC, GAO, and litigation samples, respectively). Then investment declines significantly the first year after the manipulation period (t = −3.66, −1.72, and −8.82 for the SEC, GAO, and litigation samples, respectively).

Discretionary Revenues

In this section, we confirm the primary results of the paper and explore further the temporal relation between earnings management and excess investment. To measure earnings manipulation, we use discretionary revenues, which is the residual from Equation (1) estimated separately for each industry-year. Excess investment is the residual from Equation (2), also estimated separately for each industry-year.

Table 7 presents descriptive statistics on the temporal relation between excess investment and earnings management. First, we sort excess investment and discretionary revenues into quintiles ranging from 1 (low) to 5 (high) each industry-year. Then we track the median excess investment and discretionary revenues through event time conditional on one or the other being in the highest or lowest quintile of the year 0 distribution.

Panel A of Table 7 presents median values conditional on excess investment being in the highest or lowest quintile in year 0. Conditional on excess investment being high in year 0, discretionary revenues peak in year $t-1$. By year 0, discretionary revenues begin reverting to the median. This suggests that large levels of over-investment are preceded by higher levels of discretionary revenues. Likewise, we find that discretionary revenues are lowest the year before investment is low.

Panel B of Table 7 presents median values conditional on discretionary revenues being in the highest or lowest quintile in year 0. Panel B shows a slight increase in excess investment the same year discretionary revenues are high, but there is a greater increase and the greatest level of over-investment the year after discretionary revenues are high. Similarly, excess investment is lowest the year after discretionary revenues are low. Taken together, the findings in Table 7 are suggestive that earnings management leads to excess investment.

In Table 8, we present summary statistics from the Q model of investment, augmented with measures of discretionary revenues (i.e., Equations (4) and (5)). Table 8 reveals that investment is significantly related to current and past discretionary revenues, but less so with future discretionary revenues. The relationship between discretionary revenues and investment is strongest in the prior year (t = 12.55), followed by the concurrent year (t = 11.64). When using Equation (5), the relationship between discretionary revenues and investment is strongest in the concurrent year. The small positive relationship between investment and discretionary revenues of the following year (t = 1.73) is consistent with some amount of earnings management to cover up poor returns from past over-investment. However, overall the evidence is consistent with earnings management leading to or occurring simultaneously with excess investment, not excess investment leading to earnings management. We test this statistically in Table 9.

Table 9 presents the results of Granger causality tests of excess investment and earnings management. Panel A reveals that even after controlling for past amounts of excess investment, current excess investment is significantly positively related to past discretionary revenues. Thus, earnings manipulation, as measured by discretionary revenues, causes (in a Granger sense) excess investment. This finding is confirmed when carrying out the Granger test separately by year and industry. Specifically, the Granger tests support discretionary revenues Granger causing excess investment in 89 percent of the 330 industry-year groups tested.

TABLE 7

Time-Series Relations between Excess Investment and Earnings Management

Panel A: Median Values Sorted by Excess Investment

	XINV$_t$ = High			*XINV*$_t$ = Low	
Year	***XINV***	***DREV***	**Year**	***XINV***	***DREV***
−3	0.0011	−0.0004	−3	−0.1267	−0.0035
−2	0.0223	0.0007	−2	−0.1440	−0.0038
−1	0.0952	**0.0025**	−1	−0.1786	**−0.0049**
0	**0.3566**	0.0011	0	**−0.2741**	−0.0038
+1	0.0671	−0.0024	+1	−0.1851	−0.0032
+2	−0.0030	−0.0022	+2	−0.1571	−0.0034
+3	−0.0281	−0.0021	+3	−0.1376	−0.0035

Panel B: Median Values Sorted by Discretionary Revenues

	DREV$_t$ = High			*XINV*$_t$ = Low	
Year	***XINV***	***DREV***	**Year**	***XINV***	***DREV***
−3	−0.0514	0.0027	−3	−0.0645	−0.0014
−2	−0.0550	0.0039	−2	−0.0633	−0.0020
−1	−0.0535	0.0010	−1	−0.0635	0.0032
0	−0.0450	**0.0575**	0	−0.0760	**−0.0525**
+1	**−0.0409**	−0.0024	+1	**−0.0886**	−0.0008
+2	−0.0516	0.0024	+2	−0.0766	−0.0036
+3	−0.0574	0.0015	+3	−0.0740	−0.0037

Table 7 presents median excess investment (*XINV*) and discretionary revenues (*DREV*) through time.

Panel A (Panel B) presents the median values conditional on *XINV* (*DREV*) being in the highest or lowest quintile in year 0. When conditioning on high (low) values, the highest (lowest) value over time for each variable is shown in bold.

In Panel A, *DREV* is significantly higher (lower), $p < 0.01$, in year −1 than in year +1 when *XINV* is high (low) using a two-sample Wilcoxon test.

Likewise, in Panel B, *XINV* is significantly higher (lower) in year +1 than in year −1 when *DREV* is high (low).

Panel B of Table 9 presents results from a Granger causality test of excess investment and discretionary revenues. The relation between past excess investment and current discretionary revenues is much weaker than that of past discretionary revenues and current excess investment, and the results of the Granger causality test are not as strong, though still statistically significant in the pooled sample. However, the Granger test supports excess investment leading discretionary revenues in only 12 percent of the 330 industry-year groups tested, consistent with the Granger test statistic being overstated by pooling observations across years and industries.

Taken together, the results using discretionary revenues confirm the findings from our main three samples. In addition, these results provide evidence more consistent with earnings management leading to excess investment than with earnings management being a consequence of past excess investment.

TABLE 8

Determinants of Investment with a Continuous Measure of Earnings Management

$$INV_{it} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 Q\_QRT2_{i,t-1} + \beta_3 Q\_QRT3_{i,t-1} + \beta_4 Q\_QRT4_{i,t-1} + \beta_5 CF_{it} + \beta_6 GROWTH_{i,t-1} + \beta_7 INV_{i,t-1} + \beta_8 DREV_{i,t-2} + \beta_9 DREV_{i,t-1} + \beta_{10} DREV_{i,t} + \beta_{11} DREV_{i,t+1} + \beta_{12} DREV_{i,t+2} + \varepsilon_{it} \quad (5)$$

	Mean Estimate	FM t-stat	Mean Estimate	FM t-stat
Q_{t-1}	0.25	38.30	0.22	4.49
Q_QRT2_{t-1}			−0.02	−0.39
Q_QRT3_{t-1}			−0.08	−1.32
Q_QRT4_{t-1}			0.06	0.97
CF_t	0.19	21.81	0.18	20.06
$GROWTH_{t-1}$			0.16	20.52
INV_{t-1}			0.27	18.22
$DREV_{t-2}$	0.04	7.13	0.01	2.52
$DREV_{t-1}$	0.08	12.55	0.04	7.08
$DREV_t$	0.07	11.64	0.07	11.35
$DREV_{t+1}$	0.01	1.66	0.01	1.73
$DREV_{t+2}$	0.01	0.87	0.00	0.60
Adj. R^2	0.17		0.29	

Table 8 presents summary statistics from industry-year regressions of investment. Mean Estimate is the mean of 330 separate coefficient estimates, and FM t-stat is the Fama-MacBeth t-statistic. The main and incremental intercepts are not tabulated. Variables are defined in Table 2, and each is ranked and scaled between 0 and 1 (by industry and year) before estimating the models.

VI. CONCLUSION

A large literature in accounting examines earnings management by public companies. Many studies in this literature examine whether, how, and why firms manage earnings. To date, however, this literature has provided relatively limited evidence concerning the consequences of earnings management. Our study contributes to this literature by documenting that earnings management can affect decisions within firms, in addition to those made by investors and others who rely on financial reports.

This study examines the investment behavior of firms that manipulated their earnings—firms that were investigated by the SEC for accounting irregularities, firms sued by their shareholders for improper accounting, or firms that restated financial statements. The findings are strongly consistent with the hypothesis that in the period of overstated earnings, misreporting firms over-invest in property, plant, and equipment. The over-investment is greater than that of control firms and ceases once the misreporting concludes. The findings are consistent across multiple measures of earnings management and excess investment, and multiple control samples for expected investment.

While we provide direct evidence on the timing of excess investment relative to periods when earnings management is likely, it is important to note that we do not observe the investment

TABLE 9

Granger Causality Tests of Excess Investment and Earnings Management

Panel A: Discretionary Revenues Granger Cause Excess Investment

	Full Model		Restricted Model				
	Estimate	**t-stat**	**Estimate**	**t-stat**		**p-value (pooled)**	**% Sig.**
Intercept	0.20	60.23	0.23	106.07	2 Lags:	0.00	89%
$XINV_{t-1}$	0.43	106.50	0.43	113.27	1 Lag:	0.00	92%
$XINV_{t-2}$	0.09	23.78	0.09	25.18			
$DREV_{t-1}$	0.06	17.66					
$DREV_{t-2}$	0.01	2.30					

Panel B: Excess Investment Granger Causes Discretionary Revenues

	Full Model		Restricted Model				
	Estimate	**t-stat**	**Estimate**	**t-stat**		**p-value (pooled)**	**% Sig.**
Intercept	0.48	124.06	0.48	152.48	2 Lags:	0.00	12%
$DREV_{t-1}$	−0.03	−6.31	−0.03	−6.25	1 Lag:	0.00	11%
$DREV_{t-2}$	0.06	13.50	0.06	13.75			
$XINV_{t-1}$	0.00	0.26					
$XINV_{t-2}$	0.01	2.93					

Table 9 presents results from Granger causality tests of excess investment (*XINV*) and discretionary revenues (*DREV*).
Panel A presents results of a test whether *DREV* Granger causes *XINV*, using two lags of each variable.
Panel B presents results of a test whether *XINV* Granger causes *DREV*, using two lags of each variable. p-value (pooled) is the p-value using firms pooled across years and industries. % Sig. is the percent of 330 industry/year groups where the Granger test is significant. Granger test results, but not the supporting regression results, are also presented based on one lag of each variable.

decision-making process directly. However, we believe the most plausible explanation for our findings is that earnings manipulation affects firms' internal decisions. Our tests do not allow us to distinguish whether earnings management results in distorted information for investment decision makers themselves or other parties that might have prevented the investment. These results raise intriguing questions as to exactly who is misled by earnings management—managers making investment decisions, capital providers, directors who monitor management, or some combination of the three.

The over-investment we document suggests an additional factor for the negative stock price reactions to allegations of accounting improprieties and restatements. Specifically, our findings suggest that firms manipulating their earnings also over-invest in fixed assets. These findings therefore suggest that firms deviating substantially from GAAP, as in our sample, do not only alter investors' expectations of the firm's fundamentals, but they also alter the fundamentals.

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Accounting in and for the Subprime Crisis

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ABSTRACT: This essay describes implications of the subprime crisis for accounting. First, I overview the institutional and market aspects of subprime lending with the greatest accounting relevance. Second, I discuss the critical aspects of FAS No. 157's fair value definition and measurement guidance and explain the practical difficulties that have arisen in applying this definition and guidance to subprime positions during the crisis. I also raise a potential issue regarding the application of FAS No. 159's fair value option. Third, I discuss issues that have arisen regarding sale accounting for subprime mortgage securitizations under FAS No. 140 and consolidation of securitization entities under FIN No. 46(R) associated with mortgage foreclosures and modifications. Fourth, I indicate ways that accounting academics can address the implications of the subprime crisis in their research and teaching.

Keywords: *subprime crisis; credit crunch; fair value accounting; securitization.*

Data Availability: *All data presented in this essay are available from public sources.*

I. INTRODUCTION

This essay describes implications for accounting of the subprime crisis and the credit crunch it has engendered (collectively the "subprime crisis," except when necessary for clarity).[1] These implications depend on the interplay among attributes of subprime mortgages and other positions affected by the crisis (collectively "subprime positions"), the evolution of market prices and illiquidity during the crisis, and the requirements of the applicable accounting standards. Although credit losses on subprime positions are recorded under various standards, I focus on those recorded under FAS No. 157, *Fair Value Measurements* (FASB 2006), because this standard applies to many of the positions on which large losses have been recorded and has been

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The genesis of this essay is my luncheon speech at the American Accounting Association's Financial Accounting and Reporting Section Midyear Meeting on January 26, 2008; I thank Anne Beatty for gently suggesting this opportunity to me. I also thank Dan Dhaliwal for encouraging me to write up the speech in an expanded form including implications for accounting research and teaching. Finally, I appreciate the comments of the reviewers—Leslie Hodder, Mark Trombley, and Jim Wahlen—Eddie Riedl, Jenny Tucker, and participants of presentations at the Financial Accounting Standards Board, Financial Accounting Standards Advisory Council, Harvard Business School, and Yale School of Management.

Editor's note: This paper was invited by Dan Dhaliwal.

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[1] For discussions of the economic/regulatory/public policy implications of the subprime crisis, see President's Working Group on Financial Markets (2008) and Senior Supervisors Group (2008).

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subject to considerable recent debate.[2] I also discuss issues that have arisen in accounting for securitizations of subprime assets under FAS No. 140, *Accounting for Transfers and Servicing of Financial Assets and Extinguishment of Liabilities* (FASB 2000), and for the entities used in these transactions under FIN No. 46(R), *Consolidation of Variable Interest Entities* (FASB 2003).

My intended audiences are preparers, auditors, and users of financial reports who must deal with the subprime crisis as it unfolds, and accounting standard-setters, researchers, and teachers who want to use the crisis as a learning experience for themselves and their students. Because of its severity and the significant issues it raises for accounting, I deem the crisis to be the signal researchable-teachable moment of my two-decade-plus career as an accounting academic focused on financial reporting by financial institutions for financial instruments and transactions. Because of the costs the crisis has imposed on families, firms, and the overall economy, I believe that accounting and other academics have a responsibility to understand and employ the crisis to the benefit of our disciplines, students, and society.

This essay reflects my understanding of the ongoing subprime crisis at the time of this writing in June 2008. The crisis began in earnest in February 2007 and in July 2007 ended a three-year period of unprecedented global liquidity and spawned the credit crunch. The credit crunch became increasingly broad and severe in several distinct waves over time and now extends well beyond the markets for subprime positions, although a noticeable easing of the crunch began in April 2008 largely as a result of aggressive actions taken by the Federal Reserve in March 2008. Despite this easing, it remains difficult for lenders to raise or maintain consumer loan-backed financing and thus for consumer borrowers to obtain such loans, which has reduced aggregate consumption and slowed the economy. Municipal and high-yield corporate bond issuances also remain difficult, in part due to concerns about the major bond insurers, which have now lost their AAA credit ratings. Fortunately, the bond insurers as well as major financial institutions experiencing credit losses have been able to raise capital. This has reduced but by no means eliminated the potential for further contagion, because rapid home price depreciation continues and when and where it will stop and the economic damage it will cause along the way are difficult to predict. Many parties view the subprime crisis as the worst real estate and credit crisis in the United States since the Great Depression, one with high potential to yield one of the worst overall economic crises.[3]

Notably, there have been observable feedback effects between the subprime crisis and the credit crunch. As firms announced losses on subprime positions, debt markets became more averse to holding those positions and increasingly illiquid, causing the fair values of the positions to decline further and become more difficult to measure. A primary reason for these feedback effects is the opacity of many subprime positions. This opacity is attributable in part to the complex partitioning of the risks of these positions through (re)securitizations, credit derivatives, and other financial transactions. It is also attributable in part to the fact that many subprime positions are off-balance sheet in the so-called "shadow banking system." As a result of this opacity, market participants have aggressively price-protected themselves when bidding for those positions or avoided them altogether. Many holders "capitulated" and disposed of or wrote down subprime positions to distressed valuations to remove the perceived taint from their balance sheets. Various other types of adverse feedback effects have occurred and remain of deep concern to economic policymakers.

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[2] FAS No. 157 is effective for fiscal years beginning after November 15, 2007. However, most large financial institutions early adopted FAS No. 157 in the 2007 fiscal year either voluntarily or because they were required to by FAS No. 159, *The Fair Value Option for Financial Assets and Financial Liabilities*, as a result of early adopting that standard in 2007.

[3] While I am not an economic historian, in my view the subprime crisis poses far more daunting challenges for economic policymakers than those posed by the two most analogous sets of events that have occurred since I entered accounting academia in 1982: the thrift/life insurer/junk bond crisis of the mid-late 1980s-early 1990s and the Russian debt/hedge fund crisis in the second half of 1998. Although both of these prior crises also yielded large losses on subprime positions and considerable market illiquidity, the economic problems they raised were more contained and fixable through intervention by policymakers and through firms reworking their business models and processes.

Two economic attributes drive credit losses on subprime mortgages. The first attribute—which has been well understood by market participants since the Russian debt/hedge fund crisis of 1998—is the frequency of defaults on subprime mortgages is low if mortgagors are able to obtain cash-out refinancing and high otherwise. Cash-out refinancing involves the prepayment of an existing mortgage with the proceeds of a new larger mortgage, and it can be reliably obtained by subprime mortgagors only when home prices have appreciated since the origination of their existing mortgages and debt markets are sufficiently liquid that investors are willing to hold credit-risky subprime mortgage-backed securities (MBS).[4] The second attribute—which is plainly obvious—is the magnitude of losses given default rises with home price depreciation since the mortgage origination. The almost unprecedented home price depreciation and market illiquidity in the subprime crisis have already yielded very high defaults and losses given default. Both of these variables will almost surely worsen before they improve, because of ongoing rapid home price depreciation. In addition, initial teaser interest rates on more than half a trillion dollars of adjustable-rate mortgages are scheduled to reset through the end of 2009, although the Federal Reserve has significantly reduced concerns about resets by decreasing its target federal funds rate, and thus the short-term rates to which most adjustable-rate mortgages reset, in seven increments from 5.25 percent on September 18, 2007 to 2 percent on April 30, 2008.

Credit losses on subprime mortgages flow through to the investment performance of numerous positions that are based directly or indirectly on them. These positions include subprime MBS, collateralized debt obligations (CDOs), credit derivatives and other types of financial guarantees, and liquidity support arrangements. More junior positions have been affected sooner and more severely, all else being equal, but even the most senior positions have been adversely affected.

Some parties have tried to pin the blame for the subprime crisis on fair value accounting, especially FAS No. 157, typically pointing to the obvious difficulties of measuring the fair values of subprime positions in the current illiquid markets and the feedback effects noted above.[5] This is untenable. The subprime crisis was caused by firms, investors, and households making bad operating, investing, and financing decisions, managing risks poorly, and in some instances committing fraud, not by accounting. While the aforementioned accounting-related feedback effects may have contributed slightly to market illiquidity, the severity and persistence of market illiquidity during the crisis is primarily explained by financial institutions' considerable risk overhang[6] and need to raise capital, as well as by the continuing high uncertainty and information asymmetry regarding subprime positions. Moreover, even amortized cost accounting is subject to asset impairment write-downs, and so these feedback effects likely would have been similar in the absence of FAS No. 157 and other fair value accounting standards. The best way to stem the credit crunch and damage caused by these actions is to speed the price adjustment process by providing market participants with the most accurate and complete information about subprime positions. Although imperfect, fair value accounting provides better information about these positions and is a far better platform for mandatory and voluntary disclosure than alternative measurement attributes, including any form of amortized cost accounting.

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[4] Alternatively, mortgagors can avoid default by selling their homes for at least the principal amount of their mortgages. This activity requires willing home purchasers that are able to obtaining financing, which like cash out refinancing is considerably more likely when home prices are appreciating and when debt markets are liquid. Hence, for simplicity I include this activity in "cash out refinancing."

[5] See Johnson (2008a, 2008b) and Rummell (2008). In addition, U.S. Representative Barney Frank, the chairman of the United States House of Representatives' Financial Services Committee, has asked for fair value accounting rules to be reconsidered.

[6] Gron and Winton (2001) show that financial institutions' risk overhang (i.e., risk remaining from past business decisions that cannot be eliminated due to market illiquidity) can cause them to reduce or eliminate their trading activity in positions whose risks are correlated with their risk overhang. More generally, beginning with Froot et al. (1993) the finance literature has demonstrated that market imperfections can make financial institutions risk averse and thereby reduce their trading activity.

This is not to say that guidance regarding fair value measurements in illiquid markets cannot be improved. Although FAS No. 157 provides a clearer definition of fair value and considerably expanded measurement guidance than prior GAAP, the severe market illiquidity during the subprime crisis has raised significant challenges for the interpretability of this definition and guidance. The definition reflects the notion of "orderly" transactions occurring based on the conditions that exist at the "measurement date." During the subprime crisis, this notion has been practically useless as a guide to preparers' measurements of fair value. In particular, preparers find it difficult to convince auditors and others of the reasonableness of measurements based on the "orderly transaction" element of the fair value definition when numerous fire sales by illiquid firms or capitulations by firms that want to remove the perceived taint from their balance sheets occur at considerably lower values. Thus, in practice the "orderly transaction" element of the fair value definition has been dominated by the "at the measurement date" element.

In principle, this problem could be mitigated by FAS No. 157's fair value measurement guidance, which includes a hierarchy of inputs that favors observable market inputs over unobservable firm-supplied inputs, but that ultimately requires preparers to employ "the assumptions that market participants would use in pricing the asset or liability." Unfortunately, FAS No. 157 does not include any meaningful guidance indicating when available market inputs are of such poor quality that firms should base their fair value measurements on unobservable and potentially judgmental firm-supplied inputs such as forecasts of home price depreciation. I recommend the FASB provide such guidance in a fashion that directly confronts the problem of market illiquidity, which is, after all, when fair value measurements are both most difficult to make yet most important to users of financial reports. I also recommend that the FASB require expanded disclosures of firm-supplied inputs and the sensitivity of the fair value measurements to those inputs when they are used instead of available but poor quality market inputs. As any modifications to FAS No. 157 will take time, for the duration of the crisis preparers will need to exercise considerably more than the usual professional judgment in applying FAS No. 157's fair value definition and measurement guidance to their specific circumstances. Similarly, users of financial reports will need to evaluate firms' reported fair values and disclosures with more than the usual due care.

As the successive waves of the subprime crisis have hit, firms have repeatedly and sharply revised upward their estimates of credit losses. These revisions are inevitable consequences of how the subprime crisis evolved, and they do not imply there have been any problems either with accounting standards or how preparers have applied them. However, these revisions and the high potential for further upward revisions have contributed to the aforementioned feedback effects between reported losses and market illiquidity. Needless to say, this market illiquidity has damaged our real estate and credit markets and overall economy, and it needs to be cured through means that do not simply push the problem into the future by delaying inevitable price adjustments. As always, essential components of such a cure are for firms to provide relevant, reliable, and understandable financial report information and for users of financial reports to conduct careful and dispassionate analysis of that information.

The remainder of the essay is structured as follows. In Section II, I overview the institutional and market aspects of subprime lending, focusing on those with the greatest relevance for accounting. I define "subprime" and provide a brief history of subprime lending, for simplicity focusing on mortgages throughout the essay. I describe the various types of positions created in subprime mortgage securitizations and the required accounting for those positions. I emphasize how the investment performance of these positions exhibits a binary quality that depends on subprime mortgagors' ability to obtain cash-out refinancing. I discuss how the subprime crisis has evolved in four waves that have encompassed more positions and affected positions more severely over time. In Section III, I describe the critical aspects of FAS No. 157's definition of fair value and guidance for fair value measurements. I describe the practical difficulties that have arisen in applying that definition and guidance to subprime positions in the current illiquid markets. I also discuss a potential issue regarding the application of FAS No. 159, *The Fair Value Option for Financial Assets and Financial Liabilities* (FASB 2007), in the subprime crisis. In Section IV, I discuss issues regarding sale accounting for subprime mortgage securitizations under FAS No. 140 and consolidation of securitization entities under FIN No. 46(R) associated with foreclosures and modifications of mortgages. In Section V, I indicate ways that accounting academics can address the implications of the subprime crisis in their research and teaching. Section VI contains my concluding remarks.

II. INSTITUTIONAL AND MARKET BACKGROUND

A Brief History of Subprime Lending Prior to the Crisis

During the period of very high market interest rates from the late 1970s to early 1980s, bankers and bank regulators used the adjective "subprime" to refer to the high proportion of commercial loans that yielded below the prime rate, primarily because banks wanted to retain these borrowers during this period. This is basically opposite to, and should not be confused with, the current meaning of the term.

Since early 1995, if not before, bankers and others have used "subprime" to refer to less than highly creditworthy assets (e.g., subprime mortgages) that yield higher interest rates than do prime assets with similar non-credit risks. It also refers to the parties (e.g., subprime lenders and borrowers) and transactions (e.g., subprime securitizations) involved with those assets. This meaning of "subprime" came about at that time because securitization and other markets to sell or transfer the credit risk of credit-risky assets had begun to develop in earnest. Mortgages and automobile loans were the first types of subprime loans to experience significant volume for two main reasons. First, each of these types of loans is homogeneous, and so their credit risk is amenable to statistical analysis and thus assessable by potential investors. Second, these types of loans are also collateralized by real property, which reduces lenders' exposure to subprime borrowers' high credit risk. The subprime lending industry grew rapidly over the next few years. For example, by 1997 subprime mortgage originations were about 15 percent of total residential mortgage originations in the United States ($125 billion out of $859 billion).[7]

"Subprime" quickly found its way into the popular press, typically being used as a pejorative. On September 4, 1995, Nanette Barnes referred to subprime borrowers as "potential deadbeats" (Barnes 1995). On January 28, 1996, Jane Bryant Quinn wrote even more evocatively: "Ten years ago, you might have been poison; now you're merely a 'subprime borrower'" (Quinn 1996).

Subprime lending's high credit risk became evident during the Russian debt/hedge fund crisis in the second half of 1998. This crisis was triggered by the Russian government's announcement on August 17, 1998 of a moratorium on external debt payments and a significant devaluation of the ruble. A classic "flight to quality" ensued in which debt market participants shunned credit-risky investments in favor of high credit-quality investments. Credit spreads roughly doubled in the two weeks following this announcement, with credit-risky rates rising and riskless rates falling in similar measures. The markets to sell credit-risky assets became illiquid, and so most subprime lenders could not securitize or sell their subprime mortgage inventory at a profit. Many became illiquid and had to stop originating new subprime loans. As a result, many subprime mortgagors found it hard to refinance their mortgages and so defaulted at high rates. Because home prices had begun robustly appreciating by late 1998, however, the losses given default were fairly small except for subprime home equity mortgages secured only by second liens on the mortgaged properties. Still, investors in junior subprime MBS and similar positions generally suffered significant losses during this crisis, which persisted beyond the end of 1998.

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[7] See Chomsisengphet and Pennington-Cross (2006).

While some subprime lenders went bankrupt during the Russian debt/hedge fund crisis, others survived, frequently as a result of an injection of capital from one or more outside investors. New Century Financial—which played a significant role at the inception of the current subprime crisis—is an example of the latter.

Subprime lenders learned from the Russian debt/hedge fund crisis, at least for a while. The surviving lenders typically focused on better credit-quality subprime borrowers. As a result, the industry experienced a fairly quiet, moderate-growth period through 2003. For example, subprime mortgage originations from 2001–2003 were only about 8 percent of total residential mortgage originations, barely half the percentage in 1997.

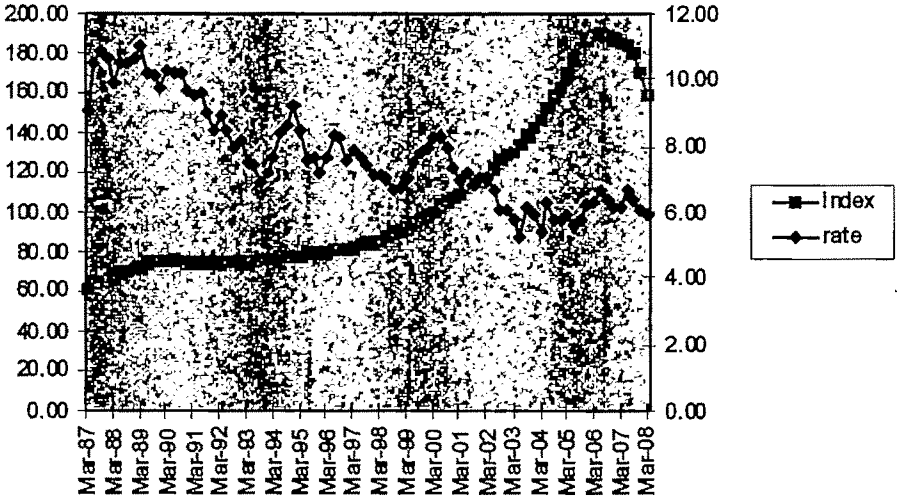
Toward the end of 2003, however, some preexisting and some new conditions combined to induce an explosion in subprime mortgage lending, which exceeded 20 percent of total residential mortgage originations in each year from 2004 to 2006 and into early 2007.

- After taking large losses on corporate positions as a result of the technology bust and recession during 2000–2002, many large commercial banks changed their strategic focus to retail banking, including subprime mortgage banking.
- As depicted in Figure 1, beginning around 1997 national home prices appreciated at a rapid rate after a protracted period of no or sluggish appreciation (e.g., annual home price appreciation was 9.7 percent from March 1997 to March 2006 compared to 1.1 percent from March 1990 to March 1997). Hence, during most of the short history of the subprime mortgage banking industry up to 2006, losses given default were fairly low even when mortgagors defaulted at high rates, as they had done in 1998 and again in the 2001 recession and its aftermath. Market participants appeared to believe that such low losses given default would continue.
- Also depicted in Figure 1, starting around 1990 fixed mortgage interest rates generally declined over time and hit a 40-year low in June 2003, keeping housing relatively affordable despite the rapid home price appreciation.
- After a period of extremely highly growth in prime mortgage originations from $0.9 trillion in 2000 to $3.4 trillion in 2003, these originations declined at the end of 2003 because mortgage prepayment and refinancing slowed as fixed mortgage interest rates rose slightly from their June 2003 low. Mortgage originators and securitizers that had become accustomed to high volume then turned to subprime mortgages.

This explosion of subprime mortgage lending was also explained in part by declining credit spreads after 2003, which allowed originators to reduce the rates they offered on subprime mortgages even as the rates on prime mortgages increased slightly. As depicted in Figure 2, Panel A, beginning in 2004 credit spreads first gradually and then sharply declined and were very low by historical standards from 2005 through mid-2007, bottoming out in June 2007 immediately prior to the beginning of the credit crunch. This resulted primarily from unprecedented liquidity in global financial markets and investors searching for higher yields driving down the price of credit risk.

Lenders' competition to originate subprime mortgages and the decline in credit spreads caused the percentage gains on sale from securitization of subprime mortgages to drop sharply over time.

FIGURE 1
S&P/Case-Shiller National Home Price Index
Freddie Mac 30-Year Fixed-Rate Mortgage Commitment Rate
March 1987–March 2008[a]



[a] Index = 100 in March 2000. These data are available free on Standard & Poor's and Freddie Mac's websites.

For example, in its Form 10-K filings, Countrywide Financial reports percentage gains on sale of 4.43 percent in 2003, 3.64 percent in 2004, 2.01 percent in 2005, and 1.84 percent in 2006. As discussed below, this decrease in profitability occurred as the credit risk of newly originated subprime mortgages increased. Because subprime lenders typically retain interests that bear the first risk of credit loss on their originated mortgages and that are considerably larger than their diminished gains on sale, most lenders' gains were considerably more than reversed through credit losses on those interests during the subprime crisis.

Attributes of Subprime Mortgages

The boundaries between prime (A) and various grades of non-prime (near prime or subprime) mortgages are vaguely determined by four main attributes of mortgagors and mortgages.

1. Mortgagors' FICO (Fair Isaac Corporation) credit scores, which primarily capture their payment histories (i.e., delinquencies, defaults, and bankruptcies) as well as their type and amount of debt outstanding and available via credit lines. FICO scores are intended to capture the probability of borrowers declaring bankruptcy, defaulting, or being more-than-90-days delinquent over a two-year horizon. FICO scores have various limitations for predicting credit losses on relatively long-term and large mortgages. For example, borrowers' debt available via credit lines improves their FICO scores by reducing the likelihood they experience credit problems over two years, even though it may enable

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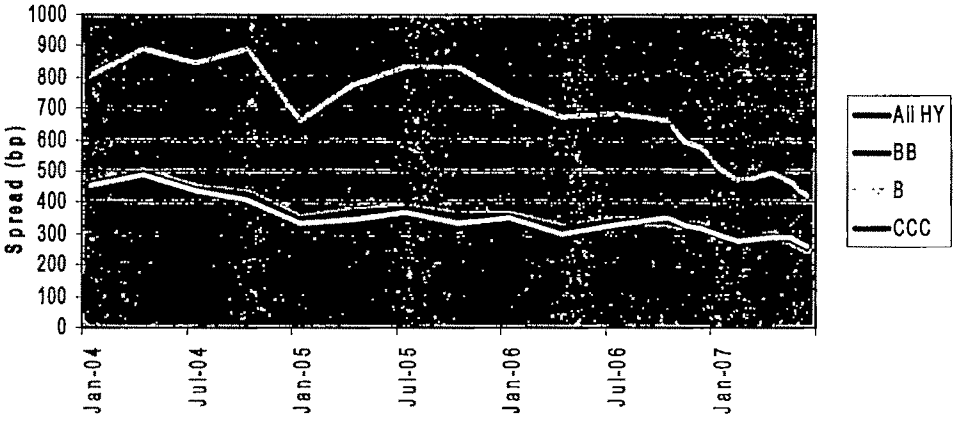
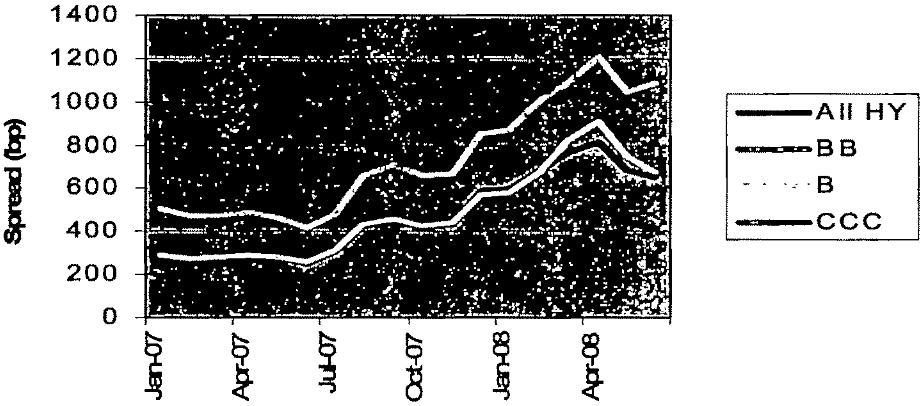
FIGURE 2
Citigroup YieldBook High-Yield Corporate Bond Credit Spreads over Treasuries
January 2004–June 2008[a]

Panel A: January 2004–June 2007



Panel B: January 2007–June 2008



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[a] BB is YieldBook's All BB, which includes BB+, BB, and B−. Similarly, B is YieldBook's All B, which includes B+, B, and B−. These data are commercially available from Citigroup's Yield Book.

them to become further indebted over the terms of their mortgages. First-time homebuyers' FICO scores may not capture their ability to make the far larger payments on mortgages than on their preexisting debts.

2. Mortgages' loan-to-value ratios, which equal the amounts of mortgage principal plus accrued points or other interest divided by the assessed values of the mortgaged properties. Combined loan-to-value ratios include piggyback second (home equity) mortgages, but do not include various types of "silent seconds," such as mortgages provided by the home seller.
3. Mortgagors' front and back debt-to-income ratios, which equal their housing-related principal, interest, insurance, and real estate tax payments (the numerator of the front ratio) or their housing-related payments plus other fixed payments, including other debt payments, alimony, child support, etc., (the numerator of the back ratio) divided by their gross income.
4. Whether the mortgagors' income and assets are documented by the borrower and verified by the lender.

Some sense for how these attributes define the boundary between prime and non-prime mortgages is provided by the following four criteria for "conforming" mortgages that may be purchased or guaranteed by Fannie Mae or Freddie Mac. Conforming mortgages typically are viewed as prime, although lower quality conforming mortgages have some subprime characteristics. For each of these four criteria, I provide rough comparables for subprime mortgages.

1. The mortgagor's FICO score must be above 660, compared to the low 600s or 500s for most subprime mortgagors.
2. The loan-to-value ratio must be less than or equal to 80 percent or the mortgagor must purchase private mortgage insurance, compared to close or equal to 100 percent (especially when piggyback second mortgages are included) with no mortgage insurance for many subprime mortgages.
3. The mortgagor's front (back) debt-to income ratio must be less than 28 percent (36 percent), compared to a back ratio of 50 percent or more for many subprime mortgages.
4. There must be documentation and verification of the mortgagor's income and assets, compared to limited or none for many subprime mortgages.

There are many different grades of non-prime mortgages. Alt-A or near-prime mortgages generally are made to mortgagors who have FICO scores well above the conforming threshold of 660 but that have less than full documentation and verification of their income and assets or higher than conforming loan-to value or debt-to-income ratios. Subprime mortgages range from A–, the best quality, to D, the worst quality. No clear industry guidelines exist as to what mixes of attributes qualify borrowers for these grades, in part because trade-offs exist among the various attributes and in part because different originators offer distinct mortgage products or serve distinct geographical areas with different housing market characteristics, foreclosure laws,[8] economic conditions, and demographics. Securitization prospectuses contain descriptions of originators' underwriting guidelines that include ranges of the four attributes that overlap considerably for the various grades of subprime mortgages. Inspection of these descriptions suggests that mortgagors' recent credit histories are a particularly important determinant of the grades of mortgages they receive; for example, a borrower with a few 30-day delinquencies in the past year may qualify for an A– mortgage, whereas one who recently emerged from bankruptcy generally cannot receive better than a C mortgage.[9]

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[8] About one-third of the states—including Arizona, California, and Michigan, which have experienced particularly high credit losses on mortgages during the subprime crisis—allow all or some (depending on the legal form of the mortgage and/or the foreclosure process) mortgagors to walk away from their homes without recourse to their general credit. State foreclosure laws are summarized at http://www.foreclosurelaw.org.

Even A– mortgages are considerably credit riskier than conforming mortgages on the criteria listed above and how they are layered. Reflecting this higher credit risk, Pennington-Cross (2003) estimates that for the 1995–1998 period (toward the end of which home prices began to appreciate at a robust rate), credit losses were five to six times higher on A– mortgages than on prime mortgages. As we have experienced, this ratio generally rises as home prices decline, at least up to the point where credit losses on prime mortgages begin to rise sufficiently, which we may yet see.

Until about 2003, a very rough rule of thumb was that each grade down the ladder added 1 percent to the yield on a mortgage. That is, an average Alt-A mortgage yielded 1 percent more than an average prime mortgage, an average A– mortgage yielded 1 percent more than an average Alt-A mortgage, an average B mortgage yielded 1 percent more than an average A– mortgage, and so on.[10] These yield differentials more than halved during the 2004–early 2007 period as competition for originations intensified and credit spreads declined.[11]

The lack of clear industry guidelines suggests that the credit risk of subprime mortgages may be poorly captured by their assigned grades and credit spreads. This is most likely to occur when new risk attributes arise or existing risk attributes are layered in new ways, both of which occurred during the 2004–early 2007 period. For example, subprime mortgage banks' financial report disclosures indicate that most of these banks dramatically increased the proportion of nontraditional mortgages that delay payments compared to traditional 15- and 30-year amortizing mortgages that they originated. Such delayed-payment mortgages include interest-only and pay-option adjustable-rate mortgages that require or allow the mortgagor to choose to pay only interest each month, hybrid adjustable-rate mortgages with low "teaser" interest rates for initial periods of two or three years, and 40-year amortizing mortgages. While risk-layering is difficult to observe from financial report disclosures, Fitch Ratings (2007) and T2 Partners LLC (2008)[12] provide evidence using loan-level data that beginning around 2004 delayed-payment mortgages were increasingly offered to and frequently chosen by mortgagors whose other attributes indicated high risk (i.e., low FICO scores, high loan-to-value and debt-to-income ratios, and incomplete or no documentation/verification of income and assets). Various other types of risk-layering also occurred.

Facilitated by lenders' lax underwriting, mortgage application fraud ("predatory borrowing") occurred to a considerable extent and in varied ways during this period. For example, mortgagors overstated their (unverified) income and assets or indicated they planned to live in the mortgaged homes, when in fact they intended to flip them for a speculative profit. Until mid-2007, mortgagors were able to increase their FICO scores by being listed as authorized users on one or more creditworthy parties' credit cards, possibly for a side payment. Mortgage brokers and real estate agents arranged mortgage originations for unqualified borrowers and fraudulent house sales to straw buyers and in order to receive commissions. Various other types of fraud also occurred.[13]

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[9] For example, New Century discusses its underwriting guidelines for various grades of mortgages in an April 14, 2006 prospectus for New Century Mortgage Securities LLC. It also indicates that because of compensating factors it makes exceptions to these guidelines for a substantial portion of its mortgages.

[10] See Hayre (1999).

[11] See Chomsisengphet and Pennington-Cross (2006).

[12] T2 Partners LLC (2008) indicates it is short Ambac and MBIA stock and has purchased credit derivatives on these firms.

[13] See Fitch Ratings (2007) for extensive discussion of mortgage application fraud.

Keys et al. (2008) provide evidence that securitized subprime loans are more likely to default than are non-securitized loans with similar risk profiles. They interpret their results as suggesting that securitization adversely affects lenders' incentives to screen loans.

The Critical Attribute of Subprime Mortgages

As discussed above, subprime mortgagors often must obtain cash-out refinancing in order to avoid default. They generally are able to do so only if home prices have appreciated since the origination of the existing mortgage and debt markets are sufficiently liquid so that investors are willing to hold credit-risky subprime MBS.

This attribute yields a binary quality to the investment performance of subprime-mortgage-related positions. When home prices appreciate and debt markets are sufficiently liquid, subprime mortgagors find cash-out refinancing easy to obtain, and so subprime mortgage defaults are both low and uncorrelated across mortgages. When a default occurs, it generally reflects something specific about the mortgagor or the mortgaged property that prevents the mortgagor from obtaining cash-out refinancing despite its general availability. Hence, a default on one mortgage has minimal implications for defaults on other mortgages. Moreover, percentage losses on the mortgages that default are relatively low because the mortgaged properties can be resold into an appreciating market. For these reasons, subprime-mortgage-related positions, even the most junior, generally experience good investment performance under these conditions.

In contrast, when home prices depreciate (or even just fail to appreciate) or debt markets are illiquid, subprime mortgagors find cash-out refinancing difficult to obtain, and so mortgage defaults are high and strongly positively correlated across mortgages. The positive correlation reflects the fact that macroeconomic factors, home price depreciation, and debt market illiquidity drive large numbers of defaults. Moreover, percentage losses on the mortgages that default are relatively high if the mortgaged property must be resold into a depreciating market. Under these conditions, most subprime mortgage-related positions, possibly even the most senior, generally experience poor investment performance.

This binary quality is related to the well-documented vintage effects in subprime mortgage defaults.[14] Subprime mortgages originated in 2006 and 2007 have defaulted at considerably higher rates than those originated in prior years. For these vintages, mortgagors' equity cushions generally declined after origination, frequently below zero, ruling out cash-out refinancing.

This binary quality implies that accounting valuations of subprime-mortgage-related positions, even when based on ideal accounting standards applied as best as possible, tend to be dramatically inaccurate *ex post* whenever the direction of home prices changes. With the recent shift from appreciation to depreciation in home prices, valuations of subprime mortgage-related positions turned out to be too high *ex post*, and they were repeatedly revised downward as the successive waves of the subprime crisis hit. These effects appeared sooner for more junior subprime positions, but when those positions were wiped out, the effects passed on to more senior positions. Opposite effects will occur whenever the current home price depreciation is replaced by home price appreciation.

Subprime Mortgage Players, Positions, and Securitizations

As depicted in Figure 3, three main types of non-mutually exclusive players assume three main types of subprime-mortgage-related positions that are accounted for differently under GAAP. The first type of player is originators, who originate mortgages and typically hold them in

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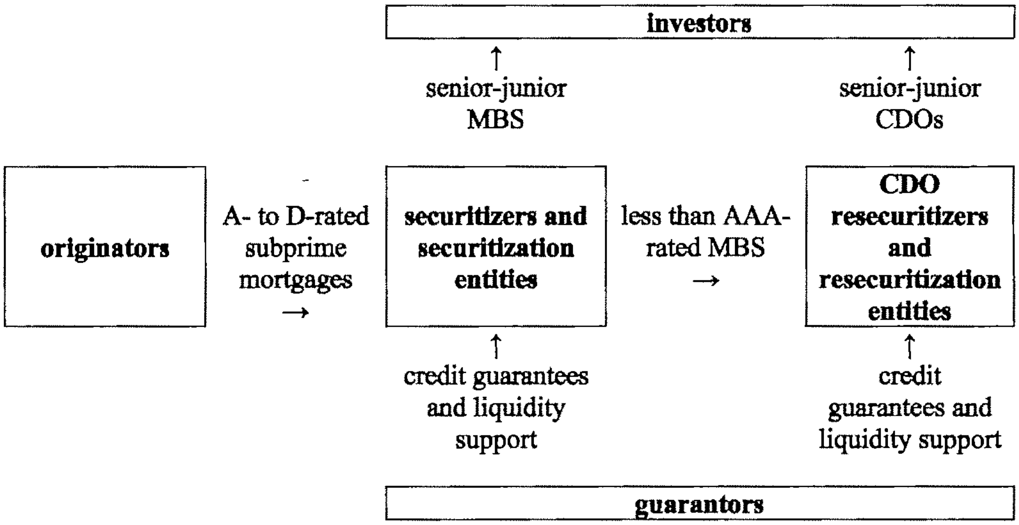
[14] See Demyanyk and van Hemert (2008).

FIGURE 3

Subprime Mortgage Players, Positions, and Securitizations[a]


investors
senior-junior MBS
senior-junior CDOs
originators
A- to D-rated subprime mortgages →
securitizers and securitization entities
less than AAA-rated MBS →
CDO resecuritizers and resecuritization entities
credit guarantees and liquidity support
credit guarantees and liquidity support
guarantors

[a] Originators may also be securitizers. Originators and securitizers may also be investors or guarantors.

inventory until they have accumulated enough to sell or securitize efficiently. During the holding period, originators bear the credit and other risks of the mortgages. For accounting purposes, originators almost always classify the mortgages as held-for-sale. Held-for-sale mortgages are accounted for at lower of cost or fair value under FAS No. 65, *Accounting for Certain Mortgage Banking Activities* (FASB 1982b).

The other two main types of players and positions arise when subprime mortgages are securitized. Before discussing these positions, I briefly describe the structural features of a typical but simplified subprime mortgage securitization.[15] The securitizer—who could be either a mortgage originator or another firm that purchased the mortgages from one or more originators—places mortgages in a bankruptcy-remote entity. The mortgages serve as collateral supporting the issuance of various tranches of MBS sequenced from most senior to most junior. The most senior tranche is sized as large as possible while still obtaining a AAA rating from credit rating agencies.[16] The most junior ("equity") tranche is unrated and sized as small as possible while still obtaining the lowest investment grade rating for the second most junior tranche.[17] The securitizer retains the equity tranche in order to credit enhance (i.e., reduce the credit risk of) the other tranches, which usually are purchased by third parties.

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[15] See Ryan (2007), Chen et al. (2008), and Ashcraft and Schuermann (2008) for detailed discussions of the variety of structural features that may be present in securitizations.

[16] In two common variations of subprime mortgage securitizations, multiple AAA-rated tranches are created to better meet the needs of distinct investor clienteles. First, to appeal to clienteles desiring longer- and shorter-duration MBS, some securitizations create multiple equally senior tranches with the rights to the cash flows generated by subsets of the securitized mortgages with different expected lives. (If the subset of mortgages supporting one of these senior tranches experiences unexpectedly high credit losses, however, then cross-collateralization across the subsets occurs to protect these tranches.) Second, to appeal to clienteles desiring very low credit risk, some securitizations create super senior tranches that are credit enhanced by less senior but still AAA-rated tranches.

Ignoring any other forms of credit enhancement (e.g., third-party guarantees), the equity tranche bears credit losses on the securitized mortgages first. If that tranche is wiped out by credit losses, then the second-most junior tranche bears the incremental credit losses on the securitized mortgages until it is wiped out, and so on until all credit losses are absorbed.

Reflecting the binary quality described above, all of the MBS tranches are likely to experience good investment performance if home prices appreciate and debt markets remain liquid. In contrast, if home prices depreciate or debt markets become illiquid, then most or all of the junior MBS tranches will be wiped out. Moreover, if home price depreciation is sufficiently large, then even the most senior tranche will experience poor investment performance.

Subprime MBS rated AA or lower often are resecuritized in CDO securitizations, which create sequenced senior-to-junior tranches of CDOs. As in the original MBS securitizations, the most senior CDO tranche typically is sized to yield a AAA rating. While it may seem odd that higher-rated CDOs can be created out of lower-rated MBS, this is possible for two reasons. First, individual MBS experiencing losses in a CDO resecuritization pool need not experience complete losses, and so the portion that is not lost is effectively allocated to the most senior CDO tranche. Second, and more importantly, these pools are constructed to include diverse sets of MBS from many different prior securitizations, and the losses on the individual MBS in the pool are expected to diversify considerably in most circumstances. However, subprime mortgages' binary quality discussed above implies that both of these reasons can fail when home prices depreciate, because losses on the underlying junior MBS will be both individually large and highly correlated. In periods of substantial home price depreciation, it is entirely possible that all of the MBS in a CDO resecuritization pool suffer near complete losses, so that even the most senior CDO issued based on those MBS loses much of its value. This in fact occurred for some CDO resecuritizations during the third or fourth waves of the subprime crisis.

CDOs may themselves be resecuritized one or more times in so-called CDO^2, CDO^3, etc., resecuritizations. The issues just discussed are accentuated for these further resecuritizations.

"Conduit" commercial paper (ABCP) and structured investment vehicle (SIV) securitizations issue asset-backed securities (ABS) with a shorter life than the securitized assets, so that the ABS have to be rolled over at the end of their life to maintain the financing for the assets. In these securitizations, a sponsoring bank usually provides liquidity support, that is, agrees to provide financing to the securitization conduit in case the ABS cannot be rolled over.

I return now to the other two main types of players and positions. The second main type of player is investors in the various tranches of MBS (CDO) securities that are backed directly (indirectly) by subprime mortgages. As discussed above, originators and securitizers are important subsets of investors. Investors usually classify these investments as trading or available-for-sale (AFS) securities under FAS No. 115, *Accounting for Certain Investments in Debt and Equity Securities* (FASB 1993) although some investors may classify some securities as held-to-maturity (HTM).[18] Both trading and AFS securities are recognized at fair value on the balance sheet, but periodic unrealized gains and losses are recorded in income for trading securities and in other comprehensive income for AFS securities. HTM securities are recognized at amortized cost. Both

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[17] In some securitizations, typically quite small non-investment-grade rated tranches are also created.

[18] FAS No. 140, paragraph 14, requires interest-only strips and other prepayment-sensitive positions for which the holder would not recover substantially all of its recorded investment upon prepayment to be classified as either trading or AFS.

AFS and HTM securities are subject to other-than-temporary impairments, in which unrealized gains and losses on the securities are recorded in net income.

The third main type of player is guarantors, broadly construed to include providers of both credit guarantees and liquidity support. Mortgage originators, securitizers, or third-party monoline insurers typically are the providers of credit guarantees. Similar to retention of junior tranches, credit guarantees credit enhance some or all of the tranches of asset-backed securities issued in a securitization. Credit guarantees can be provided in various partly or wholly substitutable ways that are accounted for differently. Credit derivatives are accounted for at fair value under FAS No. 133, *Accounting for Derivative Instruments and Hedging Activities* (FASB 1998). Other financial guarantees could be accounted for either as guarantees recognized at fair value at initiation under FIN No. 45, *Guarantor's Accounting and Disclosure Requirements for Guarantees, including Indirect Guarantees of the Indebtedness of Others* (FASB 2002), or, if provided by an insurer, as insurance contracts under FAS No. 60, *Accounting and Reporting by Insurance Enterprises* (FASB 1982a).[19] Sponsoring banks typically are the providers of liquidity support. Because liquidity support generally does not meet FIN No. 45's characteristic-based definition of a guarantee, it usually is accounted for as a loss contingency under FAS No. 5, *Accounting for Contingencies* (FASB 1975).

As discussed below, originators were the first of the three types of players to be adversely affected by the subprime crisis. This is not because they held the credit-riskiest positions—quite the contrary—but because they quickly became illiquid when they could no longer sell or securitize the mortgages they originated or their financing otherwise disappeared. After originators, the other players were affected in reverse order of seniority, beginning with investors in junior CDOs and ending with investors in senior MBS.

The Four Waves of the Crisis

The subprime crisis has, thus far, arrived in four main waves. Each wave was triggered or exacerbated by firms' announcements of increased losses on subprime positions.[20] Each successive wave increased loss severity for previously impaired positions and encompassed new positions. The last three waves dramatically decreased market liquidity first for junior subprime positions, then for senior subprime positions, and finally for other positions. I describe each of these waves in some detail.

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[19] FAS No. 60 requires insurers to accrue claim liabilities for their written financial guarantee contracts as insured events occur based on the estimated ultimate cost of settling the claims. FAS No. 163, *Accounting for Financial Guarantee Insurance Contracts* (FASB 2008b) issued in May 2008, will supersede FAS No. 60's requirements for these contracts as of the 2009 fiscal year. FAS No. 163 will require the claim liabilities for these contracts to be recorded each period at the present value of the expected claim payments (using the insurer's own assumptions based on all available information), with the risk-free rate at the initiation of the contract being the discount rate. The standard also requires considerably expanded disclosures about the measurement and evolution over time of the contract.

[20] This does not imply that a given wave would not have occurred or would have occurred in a decidedly different fashion if the specific firms making announcements of losses at the onset of the wave had not done so. Other firms likely would have announced losses on similar subprime positions at slightly later dates with similar effects on market prices and liquidity.

While problems with subprime mortgages became apparent no later than the middle of 2006,[21] the first wave really hit on February 7, 2007 when New Century Financial, the second largest subprime mortgage originator in the United States in 2006, announced that it would restate its financial reports for the first three quarters of 2006 for inadequate allowances for repurchase losses. New Century was contractually obligated to buy back mortgages it sold when the mortgages either experienced "early payment defaults"[22] or did not adhere to the representations and warranties New Century made when it sold the mortgages. Such recourse obligations are recognized at the time of sale at fair value under FAS No. 140 and are recognized subsequently at the probable and reasonably estimable losses under FAS No. 5. In its February 7, 2007 Form 8-K filing, New Century indicated that for the first three quarters of 2006 the allowance did not properly reflect the growing volume of repurchase requests or the increased expected loss on the disposition of repurchased loans. New Century also indicated that conditions deteriorated further in the fourth quarter of 2006.[23] On the same day, HSBC, the largest subprime mortgage originator, announced that its aggregate loan impairments and loss provisions would be about 20 percent above consensus analyst estimates due to deteriorating conditions in the U.S. housing market and increased subprime mortgage delinquencies.

In this wave, the market as a whole woke up to the fact that underwriting in the subprime mortgage industry had been lax and losses on subprime mortgages would likely considerably exceed those previously expected by the market and accrued for by firms. This wave primarily affected subprime mortgage banks, many of which stopped originating subprime mortgages, had large staff layoffs, or filed for bankruptcy in the following months. For example, in March 2007 New Century stopped originating mortgages because of margin calls and financing cutoffs by its lenders. Fremont General, the fifth largest subprime mortgage originator, also stopped originating subprime mortgages, and Ameriquest, the sixth largest originator, laid off about half its employees. On April 2, 2007, New Century filed for bankruptcy.[24] While this wave also affected some investors in junior CDO positions—such as two Bear Stearns' hedge funds into which Bear Stearns injected liquidity in June 2007 but that were valueless by July 2007—it did not have large repercussions for most investors in subprime positions. This wave preceded the credit crunch by several months, as can be seen by corporate bond credit spreads, after a slight and temporary increase in March and April 2007, bottoming out in June 2007, as depicted in Figure 2, Panel A.

The second wave hit and the credit crunch began in July and August 2007. No single event appears to have prompted the credit crunch, although Moody's downgrading (Standard & Poor's negative credit watch) for 399 (612) tranches of subprime MBS announced on July 10 helped to set the stage. The credit crisis clearly worsened as various investors in junior subprime positions announced losses on those positions and as various sponsors of ABCP and SIV securitizations announced the conduits could not roll over their short-term paper, thereby requiring the provision of liquidity support. Notably, on July 30 IKB Deutsche Industriebank (IKB) announced that it had to provide liquidity support to its Rhineland Funding SIV and that it also had experienced unspecified losses on its own subprime investments. With IKB financially stressed, its primary investor assumed IKB's liquidity support obligation to the SIV as well as some of the credit losses on its investments. In addition, BNP Paribas announced on August 9 that it could not reliably value its portfolio due to market illiquidity.

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[21] For example, American Business Financial Services, a subprime primarily home equity lender that originated about $1 billion of mortgages in its fiscal year ending June 2004, filed for Chapter 11 bankruptcy in January 2005. Ameriquest, the largest subprime originator in 2005, signed a multi-state settlement regarding its mortgage lending practices in January 2006 and announced a major retrenchment of its operations on May 2006. Merit Financial, which originated about $2 billion of mortgages from 2001 to 2006, fired 80 percent of its employees in May 2006. Delinquencies and early payment defaults on subprime mortgages began to rise markedly for many subprime mortgage originators around the middle of 2006. Ownit Mortgage Solutions, the 20th largest subprime mortgage originator in 2006, ceased operations in December 2006.

[22] Early payment defaults are defined in loan sale agreements. In its 2005 Form 10-K filing, New Century indicates that in its agreements early payment defaults generally are defined as default in the first or second month after loan sale.

[23] New Century subsequently announced in a May 24, 2007 Form 8-K filing that there were also errors in its 2005 fiscal year filings and in its valuation of retained residual securities.

[24] The Mortgage Lender Implode-O-Meter™ at http://ml-implode.com/ provides a comprehensive list of the subprime mortgage lenders and other firms significantly adversely affected by the subprime crisis.

This wave primarily affected investors in or guarantors of junior subprime positions. For example, as depicted in Figure 4, Panel A, Markit's ABX-HE-BBB 06-2 index—based on credit defaults swaps referenced to 20 BBB-rated ABS primarily collateralized by subprime home equity mortgages originated in the second half of 2006—implied a value for the underlying junior ABS of about 80 percent of par on July 9 that deteriorated significantly to about 43 percent of par on August 20.[25] In contrast, as depicted in Figure 5, Panel A, Markit's ABX-HE-AAA 06-2 index—which is based on credit default swaps referenced to 20 AAA-rated ABS of the same vintage—implied a value for the underlying senior ABS of about 99 percent of par on July 9 that deteriorated only slightly to about 93 percent of par on August 20. Also indicating the greater effect of this wave on more junior positions, inspection of Figure 2, Panel B indicates that during July and August 2007 credit spreads rose considerably more in both absolute and proportionate terms for CCC-rated high-yield corporate bonds than for less credit-risky BB-rated bonds.

The third wave hit and the credit crunch intensified in October and November 2007. Notable events in this wave are the announcements of large and rapidly escalating losses on subprime positions by several large financial institutions holding super senior CDOs. For example, Merrill Lynch announced a $4.5 billion loss on subprime positions, not mentioning super seniors, in its October 5 Form 8-K filing. It revised that amount upward to a $7.9 billion loss in its October 24 Form 8-K filing, with $5.8 billion for super seniors. Citigroup reported a $1.8 billion loss on subprime positions in its November 5 Form 10-Q filing, $0.5 billion of which was for super senior CDOs. It included a subsequent event disclosure of an estimated additional $8–$11 billion loss on subprime positions for events occurring after the end of the quarter on September 30. Most of this estimated loss likely was for super seniors because they constituted $43 billion of Citigroup's $55 billion subprime exposure.

This wave substantially affected investors in or guarantors of senior subprime positions for the first time and it further affected investors in or guarantors of junior subprime positions. For example, as depicted in Figure 5, Panel A, Markit's ABX-HE-AAA 06-2 index implied a value for the underlying senior ABS of about 96 percent of par on October 1 that deteriorated to about 80 percent of par on November 25. As depicted in Figure 4, Panel A, the ABX-HE-BBB 06-2 index implied a value for the underlying junior ABS of about 39 percent of par on July 9 that deteriorated to about 20 percent of par on August 20. Also indicating the greater effect of this wave on more senior positions, inspection of Figure 2, Panel B reveals that credit spreads rose proportionately more for BB-rated high-yield corporate bonds than for CCC-rated high-yield bonds during October and November 2007.

The fourth wave hit and the credit crunch moved well beyond subprime positions in late-January/March 2008. Notable events in this wave are the major bond insurers reporting losses on written credit default swaps guaranteeing subprime-mortgage-backed CDOs. In its January 22 Form 8-K filing, Ambac announced a $5.2 billion loss on these credit derivatives. In its January 31 Form 8-K filing, MBIA announced a $3.5 billion loss. These losses, while directly related to subprime positions, raised deep concerns about contagion to other positions and systemic risk, because the bond insurers guarantee about $2 trillion of municipal, asset-backed, and

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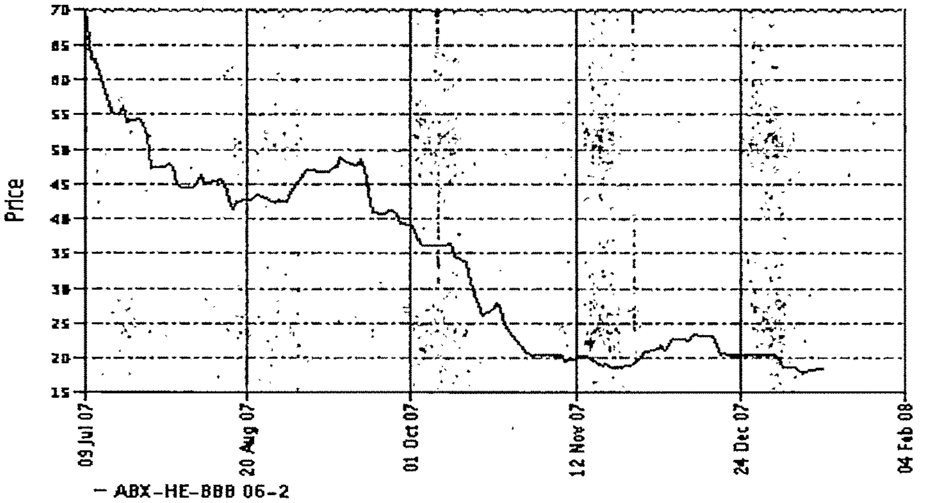
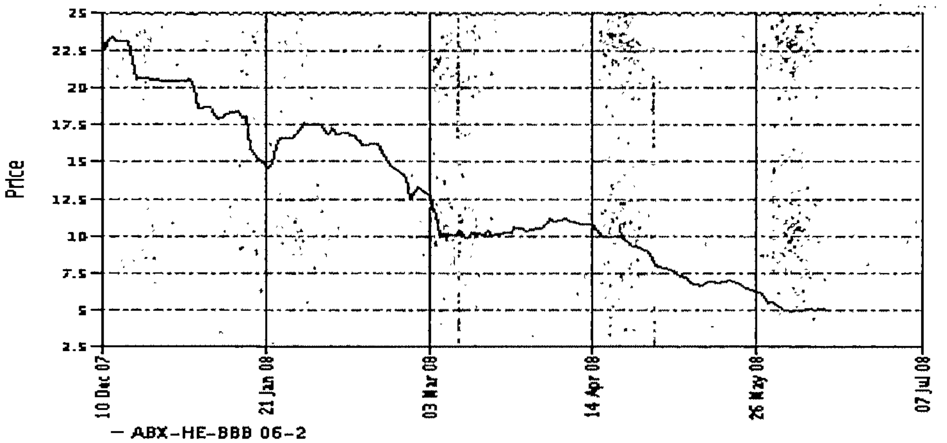
[25] See http://www.markit.com for updated graphs and more detailed description of its methodology.

FIGURE 4
Markit's ABX-HE-BBB 06-2 Index
July 9, 2007 to February 4, 2008[a]

Panel A: July 9, 2007 to Mid-January 2008



Panel B: December 10, 2007 to June 13, 2008



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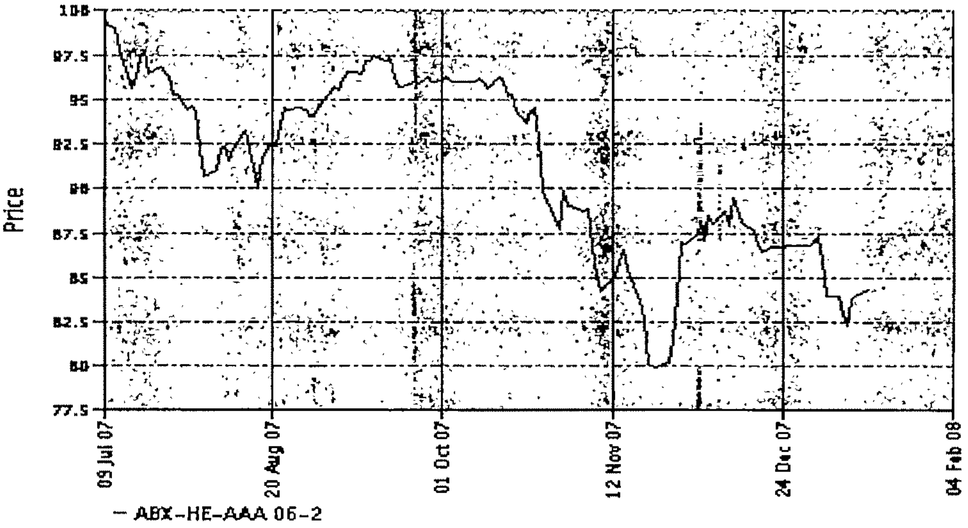
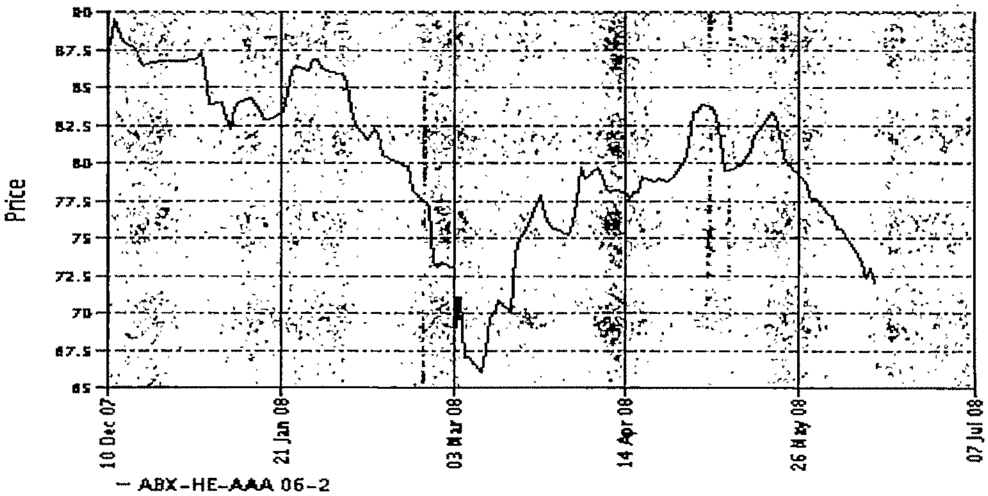
[a] Markit's ABX-HE-BBB 06-2 index reflects credit default swaps referenced to 20 BBB-rated subprime home equity MBS issued in the second half of 2006. Markit's methodology and updated graphs are available at http://www.markit.com.

FIGURE 5
Markit's ABX-HE-AAA 06-2 Index
July 9, 2007 to February 4, 2008[a]

Panel A: July 9, 2007 to Mid-January 2008



Panel B: December 10, 2007 to June 13, 2008



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[a] Markit's ABX-HE-AAA 06-2 index reflects credit default swaps referenced to 20 AAA-rated subprime home equity MBS issued in the second half of 2006. Markit's methodology and updated graphs are available at http://www.markit.com.

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other debt. Both Ambac and MBIA were able to raise external capital and thereby maintained their AAA credit ratings from Moody's and Standard & Poor's through this wave, although Standard & Poor's downgraded them to AA on June 5 and Moody's has said it is likely to do so soon. Fitch, on the other hand, lowered their ratings to AA in January for Ambac and in April for MBIA. Around this time, commercial banks announced significantly increased provisions for credit card and other consumer loans and Fannie Mae and Freddie Mac announced large losses on their credit guarantees and repurchases of defaulted securitized mortgages for the fourth quarter of 2007. This wave significantly deteriorated the financing ability of all but the most highly rated corporate, municipal, and consumer credits. Credit spreads increased yet again, with the average spread on high-yield corporate bonds more than tripling from 2.53 percent in June 2007 to 8.13 percent in April 2008, as depicted in Figure 2, Panel B.

Concern with systemic risk motivated the Federal Reserve, in concert with other central banks, to take several aggressive and virtually unprecedented actions in March 2008. In two of these actions, the Federal Reserve injected sizeable amounts of liquidity into the U.S. financial system by accepting AAA-rated subprime MBS as collateral for borrowings by primary dealers (20 large banks and broker-dealers that trade directly with the Federal Reserve). On March 11, 2008, it announced a $200 billion Term Securities Lending Facility to lend primary dealers up to $200 billion for 28-day periods through weekly auctions. On March 16, 2008, it announced a Primary Dealer Credit Facility to lend primary dealers amounts limited only by the collateral provided overnight for up to 120 days. On the same day, it induced JPMorgan Chase to purchase illiquid Bear Stearns for $2/share (which JPMorgan Chase quickly upped to $10/share to placate Bear's shareholders, many of whom were also employees) by agreeing to assume credit losses exceeding $1 billion on $30 billion of Bear's riskier positions.

Likely as a result of the Federal Reserve's actions, the credit crunch eased noticeably in April 2008, a situation that basically persists to the time of this writing in June 2008. The values of senior (but not junior) subprime positions rebounded smartly through early May, as depicted in Figures 4 and 5, Panels B, although they have fallen considerably since then. The average spread on high-yield bonds fell significantly from 8.13 percent in April 2008 to 6.48 percent in June 2008, as depicted in Figure 2, Panel B. Treasury Secretary Henry Paulson and others have suggested that this may represent the turning point in the crisis. For reasons discussed earlier, whether this will turn out to be the case remains decidedly uncertain. Financial institutions likely will continue to record very large credit losses—such as Lehman Brothers' announcement of a $3.6 billion mark-to-market loss on mostly subprime positions in its June 9, 2008 Form 8-K filing—for a considerable while longer. Moreover, recent news has not been promising. In the first two weeks of June 2008, Wachovia, Washington Mutual, and Lehman Brothers announced they had fired or demoted top executives, Standard & Poor's downgraded the credit ratings of Merrill Lynch, Morgan Stanley, and Lehman Brothers, and reports indicated that mortgage delinquencies and home foreclosure filings continue to grow at rapid rates. For example, RealtyTrac reported that home foreclosures filings in May 2008 were 7 percent greater than filings in April 2008 and 48 percent greater than filings in May 2007.

III. FAIR VALUE ACCOUNTING AND DISCLOSURES

As discussed above, specific accounting standards govern the accounting for the different types of subprime positions. While these standards employ various measurement attributes, fair value is by far the most important of these attributes, especially regarding the recognition of losses. The fair value measurement of subprime positions has involved significant practical difficulties due to high market illiquidity that developed during the subprime crisis. In this section, I discuss the application of fair value accounting for subprime positions and both required additional desirable disclosures regarding the fair values of those positions.

Scope of Fair Value Accounting

As depicted in Figure 6, the measurement attributes required by the accounting standards governing the accounting for subprime positions can be subdivided into the following broad categories. Some standards require or allow subprime positions to be fair valued on the balance sheet (e.g., FAS No. 115 for trading and AFS securities, FAS No. 133 for derivatives, FIN No. 45 for guarantees at inception, and FAS No. 159 for positions for which the fair value option is chosen). Under these standards, unrealized gains on the positions may be recorded either on the income statement (e.g., FAS No. 115 for trading securities, FAS No. 133 for non-hedge and fair value hedge derivatives, and FAS No. 159 for financial instruments for which the fair value option is elected) or in other comprehensive income (FAS No. 115 for AFS securities and FAS No. 133 for cash flow hedge derivatives).

Other standards require subprime positions to be recorded at amortized cost (possibly zero) on the balance sheet. Assets accounted for at amortized cost are subject to impairment write-downs when criteria specified in the standards are met. Assets deemed impaired based on the relevant criteria are required to be written down to fair value under some standards (e.g., FAS No. 115 for HTM securities and FAS No. 65 for held-for-sale mortgages) and to other measurement attributes that generally are higher than fair value under other standards (e.g., FAS No. 5 for liquidity support). Similarly, under FAS No. 115 unrealized gains and losses on AFS securities that previously were recorded in other comprehensive income are recorded in income when the AFS are deemed impaired.

Critical Aspects of the Definition of Fair Value

FAS No. 157 defines fair value as "the price that would be received to sell an asset or paid to transfer a liability in an orderly transaction between market participants at the measurement date." In this section, I unpack and discuss the constituent elements of this definition, describing the

FIGURE 6
Schema of Approaches to Recording Losses on
Subprime Positions under the Governing Accounting Standards[a]

Measurement Attribute	**Governing Accounting Standards and Positions**
Fair value	FAS No.115 (trading securities and available-for-sale securities) FAS No. 133 (derivatives) FIN No.45 (guarantees at inception) FAS No. 159 (positions for which fair value option is elected)
Other than fair value (but subject to impairment write-downs)	*Write down to fair value*: FAS No.115 (held-to-maturity securities) FAS No. 65 (held-for-sale mortgages) *Write down to another basis*: FAS No. 5 (liquidity support)

[a] Unrealized gains and losses on available-for-sale securities and cash flow hedge derivatives are recorded in other comprehensive income until they are realized or the position is impaired.

practical difficulties involved in applying each element. I identify a temporal slippage between two of the elements that has arisen because of the severe market illiquidity during the subprime crisis.

The definition of fair value reflects an ideal "exit value" notion in which firms exit positions through orderly transactions with market participants at the measurement date, not fire sales. In difficult times when asset liquidations may be necessary for firms to pay obligations or meet capital requirements, exit value corresponds to firms' liquidity and solvency more than do the possible alternative fair value notions of "entry value" (the price firms would pay to buy assets or receive from issuing liabilities) or "value in use" (firms' entity-specific values of assets and liabilities).[26] In particular, if all assets and liabilities on a firm's balance sheet were perfectly measured at exit value, then its owners' equity would equal the cash expected to remain if it liquidated all of those items in orderly transactions at the measurement date. Given the paramount importance of maintaining liquidity and solvency during the subprime crisis, this element of the definition of fair value has been well suited to financial report users' informational needs.

"At the measurement date" means that fair value should reflect the conditions that exist at the balance sheet date. If markets are illiquid and credit spreads (meaning the entire forward curves) are at unusually high levels, as has been the case during the subprime crisis, then fair values should reflect those conditions. In particular, firms should not incorporate their expectations of market liquidity and credit spreads returning to normal over some horizon, regardless of what historical experience, statistical models, or expert opinion indicates. While one can question this element of the fair value definition, it has considerable precedent in the accounting literature well beyond fair value accounting—notably FAS No. 5, FAS No. 107, *Disclosures about Fair Value of Financial Instruments* (FASB 1991) and SEC enforcement actions[27]—and it is hard to imagine the FASB proposing a definition of fair value without it.

An "orderly transaction" is one that is unforced and unhurried. The firm is expected to conduct usual and customary marketing activities to identify potential purchasers of assets and assumers of liabilities, and these parties are expected to conduct usual and customary due diligence. During the subprime crisis, each of these activities could have taken months because of the few and noisy signals about the values of subprime positions being generated by market transactions and because of parties' natural skepticism regarding those values. Hence, the earliest such an orderly transaction might have occurred could easily have been a quarter or more after the balance sheet date. At that time, market conditions almost certainly would have differed from those that exist at the balance sheet date, for better or worse.

The "at the measurement date" and "orderly transaction" elements of the fair value definition inevitably exhibit a temporal slippage in severely illiquid markets that yields significant practical difficulties for preparers of financial statements. Preparers must imagine hypothetical orderly exit transactions at prices reflecting current information and market conditions, even though actual orderly transactions might occur at quite distant future dates.[28] Such imagining does not take preparers far toward determining their fair value measurements, of course. Hence, they often want to solicit actual market participants for bids to help determine the fair values of positions, but they cannot do so when the time required exceeds that between the balance sheet and financial report filing dates. Moreover, any bids that market participants might provide would reflect market conditions at the expected transaction date, not the balance sheet date.

When market information about the fair values of positions is sparse, FAS No. 157 allows preparers to report internally generated fair value measurements that, in principle, could overcome this temporal slippage. Not surprisingly, however, during the subprime crisis preparers have found it difficult to convince auditors and others of the reasonableness of internally generated measurements, because numerous fire sales by illiquid firms or capitulations by firms wanting to remove the perceived taint from their balance sheets have occurred at lower values.[29] As a result, in my opinion the "orderly transaction" element of the fair value definition has effectively been trumped by the "at the measurement date" element in practice during the subprime crisis. I discuss below how the FASB can mitigate this slippage by providing additional fair value measurement guidance.

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[26] This statement does not imply that exit value is primarily a liquidity—or solvency-related notion.

[27] For example, see SEC (2004).

[28] Less plausibly, preparers could imagine that their firms began the necessary marketing and due diligence processes sufficiently before the balance sheet date (even if they did not hold the positions then).

"Market participants" are knowledgeable, unrelated, and willing and able to transact. Knowledgeable parties are not just generally sophisticated and aware of market conditions, but have conducted the aforementioned due diligence and ascertained as best as possible the fair value of the subprime position under consideration. FAS No. 157 presumes that, after conducting these activities, either market participants are as knowledgeable as is the firm currently holding the position or they can price any remaining information asymmetry. The standard does not contemplate the idea that information asymmetry between the current holder of a position and a potential purchaser or assumer of the position is so severe that markets break down altogether, as they have for many subprime positions.

The Fair Value Hierarchy

FAS No. 157 creates a hierarchy of inputs into fair value measurements, from most to least reliable. Level 1 inputs are unadjusted quoted market prices in active markets for identical items. While some accounting academics, bank regulators, and others worry that market values might be "bubble prices" or their use in accounting might have undesirable incentive or feedback effects, in my opinion pure mark-to-market measurements using such maximally reliable inputs are the rough equivalent of accounting nirvana. Even in times of normal market liquidity, this nirvana does not exist for most subprime positions, however, and so I can safely ignore such philosophical disputes in this essay.

Level 2 inputs are other directly or indirectly observable market data. There are two broad subclasses of these inputs. The first and generally preferable subclass is quoted market prices in active markets for similar items or in inactive markets for identical items. These inputs yield adjusted mark-to-market measurements that are less than ideal but usually still pretty reliable, depending on the nature and magnitude of the required adjustments. The second subclass is other observable inputs such as yield curves, exchange rates, empirical correlations, etc. These inputs yield mark-to-model measurements that are disciplined by market information, but that can only be as reliable as the models employed. In my view, this second subclass usually has less in common with the first subclass than with better quality level 3 measurements described below.

In times of normal market liquidity, many subprime positions would be fair valued using level 2 measurements. Although most subprime MBS normally are thinly traded over-the-counter, dealers generally do their best to provide bid and ask prices for these securities. There are also price and yield indices for portfolios of subprime positions available from Markit and other sources.[30] The price transparency offered by these sources substantially evaporated during the subprime crisis, however. Dealers have been reluctant to provide bid and ask quotes for subprime positions, and when they did the bid-ask spread has been very wide. Very few truly orderly transactions have occurred, and those that did typically were privately negotiated principal-to-principal transactions for which the terms and positions involved are largely opaque to market participants. Bank of England (2008) argues, rather persuasively, that Markit's indices for senior subprime MBS (but not junior subprime MBS) are well below the modeled values of those MBS even for loss scenarios more severe than are expected to occur; this likely is attributable to increased illiquidity and counterparty risk on credit derivatives. Markit has announced that there will be no indices for the first half of 2008 vintage, due to an insufficient number of securitizations.

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[29] The existence of potentially comparable but forced or semi-forced transactions distinguishes the measurement of the fair values of financial instruments during periods of market illiquidity from most other difficult fair value measurement settings, such as the measurement of the fair value of intangible assets.

[30] See Bond Market Association and American Securitization Forum (2006) for a list and descriptions of these sources.

Level 3 inputs are unobservable, firm-supplied estimates. These inputs should reflect the assumptions that market participants would use, but they yield mark-to-model valuations that are largely undisciplined by market information. Due to the declining price transparency described above, many subprime positions that previously were fair valued using level 2 inputs must now be fair valued using level 3 inputs. While many firms have been criticized in the popular press for this migration of fair value measurements down the hierarchy, it is an inevitable result of the deterioration of price transparency in the subprime crisis.

Level 3 (and to a lesser extent Level 2) inputs often are derived from statistical or other analysis of historical data. Historical data is only useful for measuring fair values to the extent that the future is expected to be similar, or at least capable of being related, to the past. For example, home price depreciation is a critical underlying level 3 input for measuring the fair value of subprime-mortgage-related positions, as it drives the frequency and correlation of defaults and the percentage losses given default. Most of the historical data to date (and *a fortiori* up to earlier points in the subprime crisis) reflect an extraordinarily favorable period of robust home price appreciation and thus few and uncorrelated defaults and small percentage losses given default. Hence, these data are of little use for the purposes of measuring the current fair values of subprime-mortgage-related positions. Instead, firms must develop models to forecast home price depreciation and its effects on the fair values of these positions.

Needless to say, such forecasts are very difficult to make. For example, in late 2007 reasonable firms could have forecast national home prices declining by a relatively modest amount such as 10 percent, declining gradually by say 25 percent to an interest-rate-adjusted historical trend line, declining sharply to that trend line, overshooting the trend line by a significant amount, or following any number of other paths. While the first of these options is now off the table, considerable uncertainty remains. The fact is, nobody really knows where home price depreciation will end up, how it will get there, or the losses it will cause along the way.

Numerous other level 3 inputs are important in the valuation of subprime positions but are hard to estimate. For example, the discount rate (riskless rate plus credit spread) is a critical input into the valuation of originators and securitizers' retained equity tranches and other residual or subordinated securities. Because these positions trade only rarely, this discount rate invariably is an internal estimate.[31]

In principle, the previously identified temporal slippage between the "orderly transaction" and "measurement date" elements of the fair value definition could be mitigated by the FASB providing additional guidance as to what indicators, if present, would suggest that available level 2 inputs are of such poor quality that firms should base their fair value measurements on unobservable and potentially judgmental level 3 inputs such as forecasts of house price depreciation. I recommend that the FASB provide such guidance, and that it also require expanded disclosures of level 3 inputs and the sensitivity of fair value measurements to level 3 inputs when they are used instead of available but poor quality level 2 inputs. Indeed, given the poor quality market signals currently being generated, I believe level 3 fair value measurements supported by disclosures of critical inputs and the sensitivity of the measurements to the inputs often would be considerably more informative to users of financial reports than poor quality level 2 fair value measurements.

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[31] Assuming perfect and complete markets, in theory the discount rate should be consistent with weighted-average cost of capital logic. For example, if interests in the securitized assets are divided into sold versus retained interests, then the discount rate for the securitized assets should equal the weighted-average of the discount rates for the sold and retained interests, with the weights reflecting the relative values of the sold and retained interests. This logic does not by itself identify the discount rates for retained interests, however. Assuming that the value and discount rates for the securitized assets and the sold interests are known, this logic only determines the product of the value and the discount rate for the retained interests. The same product results from proportionately raising the value and lowering the discount rate for the retained interests, and *vice versa*.

Required Disclosures

Subprime positions are subject to the disclosure requirements of the governing accounting standards (e.g., FAS No. 115 for securities) that, to conserve space, I do not mention here.[32] Instead, I discuss three overarching disclosure requirements of particular relevance to subprime positions during the subprime crisis.

First, FAS No. 157 requires disclosures of fair value measurements by level of the hierarchy. The required disclosures are considerably more detailed for level 3 fair value measurements than for higher level measurements. In particular, firms must provide quantitative reconciliations of beginning and end-of period level 3 fair value measurements, distinguishing total (realized and unrealized) gains and losses from net purchases, sales, issuances, settlements, and transfers. The line-item location of gains and losses on the income statement must be indicated. Qualitative descriptions of measurement inputs and valuation techniques must be provided. These disclosures make the effects of level 3 measurements on the financial statements considerably more transparent than they would have been under prior GAAP. They allow users of financial reports to isolate and separately analyze unrealized gains and losses based on level 3 measurements.

Second, SOP 94-6, *Disclosure of Certain Significant Risks and Uncertainties* (AICPA 1994), requires disclosures regarding an uncertain estimate such as a fair value when it is reasonably possible the estimate will change in the near term (one year or less) and the effect of the change would be material to the financial statements. The disclosure should indicate the nature of the uncertainty. Disclosures of the factors that cause the estimate to be sensitive to change are encouraged but not required. In March 2008, the SEC Senior Assistant Chief Accountant sent a letter to approximately 30 firms with significant level 3 fair value measurements, further nudging them to provide quantitative disclosures of unobservable level 3 inputs in the Management Discussion and Analysis section of their filings (SEC 2008).

No GAAP actually require firms to provide quantitative disclosures of level 3 inputs or of the sensitivities of fair value measurements to those inputs, however. In the absence of such disclosures, during the subprime crisis I have found reported level 3 measurements to be very difficult to interpret for a given firm and to compare across firms. To enhance their interpretability, I suggest the FASB consider requiring firms to make such disclosures.

Third, SAS No. 1, Section 560, *Subsequent Events* (AICPA 1972), requires firms to disclose type 2 subsequent events, i.e., events that occur between the balance sheet date and the financial report filing date, when those events render their financial statements misleading as of the filing date. All four waves of the subprime crisis began between calendar-year firms' fiscal quarter ends and filing dates. Hence, each wave likely triggered type 2 subsequent event disclosure requirements for firms holding significant subprime positions of the types materially affected by that wave. Citigroup's previously discussed third quarter 2007 subsequent event disclosure for its super senior CDOs is a good example.

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[32] See Ryan (2007) for detailed discussion of these required disclosures.

Fair Value Option

FAS No. 159 allows firms to elect to account for individual financial instruments at fair value on the balance sheet with unrealized gains and losses recorded in income. This election may be made upon the adoption of the standard or at the inception of the instruments. Many securities firms have elected the fair value option for the liabilities of their consolidated securitization entities. They have done so primarily because they are required by industry or other GAAP to record the entities' assets at fair value, and so fair valuing the entities' liabilities yields symmetric accounting. In general, such symmetry is desirable, because offsetting gains and losses on these economically matched positions are recorded in the same period.

The concern arose during the subprime crisis, however, that these firms may have the incentive to voluntarily provide either credit support (i.e., moral recourse) or liquidity support to their securitization entities. If the firms provide moral recourse, then they will bear *previously* incurred losses on the entities' assets without benefiting from offsetting gains on the entities' liabilities, and so symmetric accounting that does not incorporate the expected losses from the provision of moral recourse would be undesirable. In contrast, if the firms provide voluntary liquidity support on market terms at the time of provision (so that liquidity support is entirely distinct from moral recourse), then they will only bear losses on the entities' assets incurred *after* the time of provision, and so symmetric accounting up to that time would still be desirable.

I am unaware of any examples of firms providing moral recourse during the subprime crisis, although this may be because it is hard to observe. As discussed in detail by Chen et al. (2008), the incentive to provide moral recourse is strongest in revolving loan (home equity line of credit and credit card) securitizations due to contractual provisions requiring early amortization (accelerated payoffs to investors) when the securitized assets under-perform. Moreover, issuers can provide moral recourse opaquely in these securitizations, in part because they are required to add (subtract) assets from the securitization trusts as the borrowers pay off (borrow more on) their lines of credit on an ongoing basis, and in part because master trusts are used to conduct multiple securitizations. In contrast, a number of financial institutions have observably provided voluntary liquidity support to their ABCP or SIV conduits during the subprime crisis when these conduits could not roll over their paper.

IV. ACCOUNTING FOR SECURITIZATIONS AND RELATED ENTITIES

Securitizations

Two issues have arisen during the subprime crisis regarding firms' accounting for securitizations. First, FAS No. 140 allows sale accounting for securitizations of financial assets only if issuers surrender legal and effective control over the assets, as defined in paragraph 9 of the standard. Courts in Ohio and elsewhere have recently ruled that issuers' assignments of mortgages to securitization entities may have been deficient because the entities' trustees or servicers did not (and perhaps cannot) prove the entities have legal standing to foreclose on the mortgaged properties.[33] I suspect this problem results in part from firms' haste in conducting securitizations due to the high volume of mortgage originations in recent years.[34] Because securitizations of conforming mortgages are highly standardized, this problem is considerably more likely to arise for securitizations of prime nonconforming, Alt-A, and subprime mortgages. These court rulings then the American Securitization Forum would not have had to interpret "reasonably foreseeable." Although it may be a good thing, given current circumstances, to provide securitization issuers, trustees, and servicers with incentives to modify large amounts of subprime mortgages, and the SEC (although not its Chief Accountant) has the right to write new or modify existing accounting standards, I think Mr. Hewitt should have indicated he was providing an exemption to rather than interpreting FAS No. 140.

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[33] The most notable of these are 14 foreclosure cases brought by plaintiff Deutsche Bank in its role as securitization entity trustee. Judge Christopher A. Boyko, United States District Court, Northern District of Ohio, Eastern Division, dismissed the cases without prejudice on October 31, 2007. Judge Boyko concluded that Deutsche Bank did not provide evidence that the securitization entity owned the mortgages (Boyko 2007). Whether Deutsche Bank is able to provide such evidence remains unclear.

[34] It likely also results in part from lawyers' haste in filing foreclosure claims, a correctable problem.

The desire to modify the terms of subprime mortgages, supposedly passive financial assets, held by QSPEs raises the question of whether the notion of a QSPE should be eliminated. At its April 2, 2008 board meeting, the FASB decided to eliminate that notion. At its April 9, 2008 board meeting, however, the FASB decided to add a "passive interest" notion to FIN No. 46(R); this notion may play a similar role as QSPEs in allowing firms to retain risk in their securitization and other entities while avoiding consolidation.

Consolidation of Securitization Entities

The subprime crisis has raised several issues regarding consolidation of securitization entities. To state these issues clearly, I first describe the relevant aspects of FIN No. 46(R), the rule that governs consolidation of variable interest entities (VIEs). VIEs include virtually all securitization entities, although FIN No. 46(R) exempts QSPEs from consolidation by the issuer and in most cases by other parties, and so only non-QSPE entities are subject to consolidation.

VIEs are entities with equity ownership that either is insufficient to finance their operations or does not involve the usual control rights or financial interest associated with equity. Because of the insignificance of their equity ownership, consolidation of VIEs is based instead on parties' holdings of variable interests—contractual, ownership, or other pecuniary interests in an entity that change with changes in the fair value of the entity's net assets exclusive of variable interests. Examples of variable interests include equity and debt securities, profit and revenue sharing agreements, and financial guarantees. As indicated in paragraph B10 of FIN No. 46(R) and in FASB Staff Position (FSP) FIN No. 46(R)-5, *Implicit Variable Interests under FASB Interpretation No. 46 (revised December 2003)*, variable interests may be implicit, that is, arise indirectly or from parties' economic incentives. For example, the incentive to provide moral recourse to a securitization entity may be an implicit variable interest. A party that holds variable interests that bear more than half of the expected losses or expected residual returns of the VIE's net assets exclusive of variable interests, referred to as the primary beneficiary, must consolidate the VIE.

The decision as to which party, if any, is the primary beneficiary of a VIE is made only at the inception of the entity and at subsequent reconsideration events defined in paragraph 15 of FIN No. 46(R), not each quarter. Reconsideration events differ for the primary beneficiary and for other parties. For the primary beneficiary, reconsideration events occur when it disposes of its variable interests to unrelated parties or when the VIE issues new beneficial interests to other parties. For other parties, reconsideration events occur when they acquire additional variable interests in the VIE. Importantly, under paragraph 7 of FIN No. 46(R) the incurrence of losses by a VIE is not by itself a reconsideration event, even though it may change the party that bears the VIE's incremental losses and residual returns. For example, assume the primary beneficiary of a VIE holds a first-loss position in a VIE. After that position is wiped out by losses, the second-loss position will bear the incremental losses and residual returns, but the party holding that position will not need to reconsider whether it is the primary beneficiary unless it acquires additional variable interests.

The subprime crisis has caused many firms to dispose of or to acquire subprime positions that are variable interests in VIEs. Due to the occurrence of these reconsideration events, it is considerably more likely than usual that the primary beneficiaries of VIEs have changed.

Two types of events have occurred with some frequency during the crisis for which it is unclear whether they constitute reconsideration events. The first type involves the provision of contractually required liquidity support to ABCP and SIV securitization conduits. Liquidity support may be a variable interest depending on its terms. At the inception of an ACBP or SIV securitization, a party that only provides liquidity support to the conduit is unlikely to be its primary beneficiary. This is partly because the provision of liquidity support is a low probability event and partly because liquidity support usually is provided on market terms at the time of provision. If the conduit cannot roll over its ABS, then the provider of liquidity support may be contractually required to buy those securities, which are variable interests. The question is whether the required purchase of the securities constitutes the acquisition of *additional* variable interests, which would be a reconsideration event, or the fulfillment of *existing* variable interests, which would not. Logically, the provider of contractual liquidity support is in an economically identical position immediately before it provides the required liquidity support and immediately afterward. Reflecting this logic, my view is that the contractually required provision of liquidity support generally is not a reconsideration event, although FIN No. 46(R)'s language certainly can be read the other way.

The second type of event is one that changes a party's economic incentives and thereby changes the significance of the party's implicit variable interests in a VIE. For example, an issuer in a securitization may have incentives to provide moral recourse under certain conditions. At the inception of the securitization, those conditions generally are expected to occur with a very low probability, and so the issuer is unlikely to be the primary beneficiary of the securitization entity at that time. The probability that the issuer will provide moral recourse, however, rises with market illiquidity. The question arises whether increasing market illiquidity constitutes a reconsideration event. My view is it does not, because the realization of bad luck is not a reconsideration event. However, the indirect or non-contractual nature of implicit variable interests and the lack of specific language in FIN No. 46(R) regarding reconsideration events for implicit variable interests make it difficult to determine what, if anything, could constitute a reconsideration event for those interests.

Reflecting these ambiguities and other issues regarding reconsideration events, at its April 9, 2008 board meeting the FASB decided to eliminate that notion and instead require firms to evaluate each quarter whether they should consolidate the VIEs with which they are involved.

V. IMPLICATIONS FOR ACCOUNTING RESEARCH AND TEACHING

As discussed above, the subprime crisis constitutes a signal researchable-teachable moment. It raises significant questions about the applicability of FAS No. 157's fair value measurement guidance in illiquid markets, about sale accounting for securitizations and QSPE status for securitization entities when the securitized assets may need to be actively managed under FAS No. 140, and about consolidation of non-QSPE entities under FIN No. 46(R). It also raises significant questions about how preparers of financial statements have applied this guidance in practice and about financial report users' ability to make investment and other decisions. In this section, I indicate avenues along which accounting academics can address these questions in our research and teaching.

Accounting is used for firm valuation, contracting, and governance purposes, among others. The subprime crisis raises issues for accounting research and teaching related to all of these purposes. To keep the length of this section manageable, however, I focus on two big-picture issues related to the valuation purpose of accounting for which the subprime crisis can either provide the data for sharply focused empirical-archival research or directly motivate experimental-behavioral research. I view the two research approaches as highly complementary ways to address these issues for two main reasons. First, many of the issues will be difficult to address using empirical-archival methods because of lack of data availability or selection/control problems, but amenable to experimental-behavioral research. I indicate below where this is likely to be the case. Second, many of the issues could be partly explained by psychological phenomena that primarily have been addressed using experimental-behavioral methods. I do not discuss psychology and related research opportunities as it is outside of my expertise, although I note that Koonce and Mercer 2005 argue that psychological theories can also motivate empirical-archival research.

I also think there is much to be learned about these issues from careful analysis of cases examining individual firms' financial reporting decisions. We are considerably more likely to know an individual firm's exposures to subprime positions than to be able to control for differences in these exposures across firms in empirical-archival analysis. Using a not-too-hypothetical case, I indicate ways these issues can be made salient in the classroom.

Lead-Lag Properties of Fair Value Measurements in Illiquid Markets

The first issue is whether and to what extent firms' reported fair value losses for subprime positions have reflected the worsening of market signals up to the balance sheet date versus increased the adverseness of future market signals. As discussed above, some parties have tried to pin the blame for the subprime crisis on fair value accounting, basically arguing that firms' reporting of fair value losses on subprime positions has led to otherwise avoidable market panic and illiquidity yielding further losses that do not reflect the expected payoffs on these positions. While in my opinion this position is untenable, in principle it implies a set of hypotheses that are amenable to test.

In particular, we can empirically test whether and to what extent reported fair value losses have reacted to versus driven movements in observable (e.g., Markit's) market indices, both of which may have occurred. This essentially involves tests of the direction of Granger causality. A simple way to conduct these tests would be to estimate a "reverse" regression of reported fair value losses on lagged and lead changes in market indices. Significant positive coefficients on lagged changes in market indices would be consistent with fair values summarizing prior market information. In contrast, significant positive coefficients on lead changes in market indices would be consistent with feedback effects. Such feedback effects are not necessarily undesirable, however, as they could simply involve the acceleration of losses that would have eventually occurred anyway. Hence, to deem feedback effects undesirable, we would also have to find that changes in the market indices around the announcement of losses subsequently reversed in a predictable fashion. I suspect it will be difficult to devise powerful and interpretable tests of reversals of market indices even if they exist, primarily because we do not have enough independent observations to distinguish predictable reversals from unpredictable luck. For example, the actions taken by the Federal Reserve in March 2008 benefited many of the firms that had been adversely affected by the crisis. Hence, this question is likely to be more fruitfully addressed through experimental-behavioral research.[38]

We could also conduct similar tests using indicators of market illiquidity (e.g., high bid-ask spread and low trading volume) in place of market indices to determine whether and to what extent reported fair values have reacted to versus driven market illiquidity, both of which may have occurred. Because most subprime positions trade over-the-counter and these markets have almost entirely broken down, I again suspect that this question is likely to be more powerfully addressed through experimental-behavioral research. If we find evidence of such feedback effects with respect to market illiquidity, then we could investigate whether this illiquidity is an inevitable result of a price discovery process amid high levels of uncertainty or somehow attributable to fair value accounting.

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[38] Bloomfield et al. (2006) is an experimental-behavioral study that finds feedback effects resulting from a particular misuse by investors of unrealized fair value gains and losses recorded in accumulated other comprehensive income. These feedback effects are of an entirely different nature from those that appear to have occurred in the subprime crisis.

The above analyses could be refined in various ways. First, we could test whether the lead-lag properties of reported losses differ for level 2 measurements estimated using the poor quality but observable market signals currently being generated and level 3 measurements estimated using unobservable firm supplied inputs. Second, we could test whether the properties of level 3 measurements change if firms disclose these inputs and the sensitivity of the measurements to those inputs or if firms have a richer information environment (e.g., greater analyst following or a higher proportion of sophisticated investors such as institutions).

Causes of and Cures for Opacity

The second issue is whether and how firms' economic leverage and risk arising from off-balance sheet subprime positions (e.g., the sold tranches of subprime mortgage securitizations accounted for as sales) and on-balance sheet but concentrated-risk subprime positions (e.g., retained junior tranches or credit derivatives) are assessable from their financial reports and other publicly available information. As noted above, the opacity of firms' subprime positions is a likely reason for the feedback effects that appear to have occurred as firms reported fair value losses on those positions.

To address this issue, which is largely unexplored in accounting research, researchers will need to determine both the causes and cures of the opacity of subprime positions. Some clues about what causes opacity are provided by two recent studies examining banks' loan securitizations, for which banks' regulatory filings provide relatively detailed (though by no means complete) and standardized information. Chen et al. (2008) find that attributes of these securitizations—most importantly, banks' retention of credit-enhancing interest-only strips, a type of residual interest—are significantly associated with banks' unsystematic risk. Cheng et al. (2008) find that the magnitude of these loan securitizations is associated with three measures of banks' opacity: bid-ask spread, trading volume, and analyst forecast dispersion. These studies suggest that retained positions with highly concentrated risks and off-balance sheet positions are particularly opaque.

The obvious cure for opacity is disclosure. This could involve requiring firms to identify their on- and off-balance sheet subprime positions, the economic attributes of positions such as risk concentration, and any hedging or other risk management of those positions. Such disclosures would be similar to those now required for derivatives under FAS No. 161, *Disclosures about Derivative Instruments and Hedging Activities—An Amendment of FASB Statement No. 133* (FASB 2008a). It could also involve requiring firms to disclose the critical inputs underlying reported fair value measurements and the sensitivity of the measurements to those inputs.

I think this issue also is likely to be best addressed through experimental-behavioral research, for the following reasons. Empirical-archival researchers can address the effect of specific types of disclosures on opacity only to the extent that firms provide these disclosures. Potentially desirable disclosures may have been provided by few or no firms. Moreover, firms that enhance their disclosures often do so in the periods they report material fair value losses and/or make significant changes to their positions. As a consequence, empirical researchers likely will find it difficult to untangle the reduced opacity that comes from enhanced disclosures from the generally bad news those disclosures convey. In contrast, experimental-behavioral researchers can hold positions and news constant while manipulating disclosures to determine which types best cure opacity.

A Not-Too-Hypothetical Case

Assume the following facts. A large commercial bank sponsors CDO resecuritizations of subprime MBS and retains only the super senior tranches, similar to Citigroup and Merrill Lynch. Moral recourse is not an issue. The bank is preparing its Form 10-Q filing for the third quarter of 2007, which requires it to estimate the fair value of those tranches as of the balance sheet date September 30, 2007. The bank also must consider making subsequent event disclosures indicating the approximate fair value of the tranches as of the filing date if this amount is materially different from the reported fair value.

The bank's filing date occurs after Citigroup and Merrill Lynch's Form 10-Q filings on November 5 and 7, 2007, respectively, so it has the benefit of observing both these filings. Citigroup recorded a 1 percent loss on its super senior positions in the third quarter and provided a subsequent event disclosure indicating a further 22 percent loss on its total subprime positions (presumably the percentage loss on the super seniors was lower). Merrill Lynch recorded an 18 percent loss, indicating the percentage loss on super senior tranches was 8 percent for CDO securitizations collateralized by high-grade (roughly AA-rated) subprime MBS, 38 percent for CDO securitizations collateralized by mezzanine (roughly BBB-rated) subprime MBS, and 57 percent for CDO securitizations collateralized by other CDOs. Merrill Lynch did not include a related subsequent event disclosure in its filing. The bank is also generally aware that other firms, such as Ambac, MBIA, and AIG, have significant exposures to super senior CDO positions but have not yet announced any losses for the quarter.

In addition to this information, the bank observes Markit's ABX-HE-BBB 06-2 and ABX-HE-AAA 06-2 indices depicted in Figures 4 and 5. The BBB (AAA) index corresponds roughly to the value of the collateral underlying super senior CDOs collateralized by mezzanine (high-grade) subprime MBS. The bank is generally aware that these indices have been driven down by very large bid-ask spreads for the credit default swaps underlying the indices. Half of the bank's super senior tranches are collateralized by each of high-grade and mezzanine subprime MBS. The bank believes that almost all of the value of the collateral will accrue to the benefit of the super senior tranches (i.e., the other tranches of the CDO securitizations are basically worthless). The face value of each super senior tranche is 75 percent of the face value of the underlying subprime MBS collateral.

The bank also estimates its own level 3 fair values for its super senior tranches, for which the critical input is home price depreciation. It assigns the following probabilities to five possible levels of home price depreciation and associated percentage losses on its holdings of super senior CDOs.

Home Price Depreciation	**Estimated Percentage Loss**	**Probability Occurs**
10%	0%	20%
15%	5%	40%
20%	20%	25%
25%	40%	10%
30%	80%	5%

Thus, the most likely (expected) percentage loss is 5 percent (15 percent).

The facts in this case afford many avenues for fruitful classroom discussion.

- What are the absolute and relative strengths and weaknesses of fair valuing the bank's super senior tranches using the following approaches:

  - the percentage losses recorded by Citigroup and Merrill Lynch,
  - the Markit indices, and
  - its level 3 valuation model?
- Does FAS No. 157 allow the bank to use the level 3 valuation approach given the availability of imperfect level 2 inputs such as the Markit index? If so, then what aspects of the facts in the case justify this?
- For each of the valuation approaches, what disclosures would make the approach most informative to users of financial reports
- If the bank chooses the level 3 valuation approach, then what factors should/might play into the bank's decision whether to voluntarily disclose the 80 percent loss that would result from 30 percent home price depreciation given the low assessed probability of that outcome?

VI. CONCLUDING REMARKS

Like all of the severe crises that have periodically beset our remarkably flexible economy, the subprime crisis is not and could not be the fault of any one set of parties. The entire economic ecosystem failed to appreciate the risks of the rapid growth in risk-layered subprime mortgages, the inevitable reversal of home price appreciation, and unprecedented global market liquidity. These factors combined to enable all-too-human undisciplined behaviors in lenders, borrowers, and investors, almost all of whom were unquestioningly optimistic for as long as the sun shined upon home equity. Economic policy, bank regulation, corporate governance, financial reporting, common sense, fear of debt and bankruptcy, and all of our other protective mechanisms were insufficient to curb these behaviors. The process played out exactly as Keynes (1936) described the behaviors underlying upswings in economic cycles:

> Even apart from the instability due to speculation, there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as the result of animal spirits—a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities.

This passage also captures how divorced the process was from the economic and statistical concepts, such as fair value, that underlie accounting.

Accounting, fair value or otherwise, will never eliminate such behaviors. It can primarily play two roles. It can provide periodic financial reports that inform relatively rational and knowledgeable market participants on an ongoing basis, thereby making firms' behaviors more observable and allowing market participants to mitigate the adverse effects of those behaviors through their investment and other decisions. It can provide a common information set upon which market participants can recalibrate their valuations and risk assessments when the economic cycle turns. In my view, fair value accounting plays an essential part in both of these roles, but especially in allowing such recalibrations to occur as quickly and efficiently as possible, as it is now doing in the subprime crisis. By comparison, any form of historical cost accounting would drag out these recalibrations over considerably longer period, likely worsening the ultimate economic cost of the crisis.

This is not to say that fair value accounting and other aspects of GAAP have worked perfectly during the subprime crisis. The crisis has made clear that financial statement preparers need additional guidance regarding how to calculate fair values in illiquid markets. Users of financial reports need better disclosures about the critical level 3 inputs and how sensitive fair value measurements are to those inputs. Accounting standard-setters need to consider what guidance and disclosures to require. Preparers need to provide these disclosures in an informative fashion, and users must analyze them carefully and dispassionately. Accounting researchers and teachers can contribute to all of these processes. Indeed, for all of us who care about accounting and its role in our economy, there is much work to be done.

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Legal Liability Coverage and Voluntary Disclosure

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ABSTRACT: This paper examines whether legal liability coverage, as measured by excess Directors' and Officers' (D&O) liability insurance coverage and excess cash for indemnification, is associated with the quantity and the quality of a firm's voluntary disclosures. Using Canadian firms whose D&O insurance data are publicly available, I find that firms with higher excess coverage are less likely to report bad news forecasts for the sample firms that are cross-listed in the U.S., and that the number of bad news forecasts decreases for large cross-listed sample firms having high litigation risk. The results are consistent with the litigation cost argument for the disclosure of bad news. I also find that higher excess liability coverage leads to disclosures of more precise bad news for the cross-listed sample firms and less timely disclosures of bad news for large cross-listed sample firms. Further, excess cash for indemnification is a more significant determinant of disclosure decisions.

Keywords: *Directors' and officers' liability insurance; indemnification; litigation costs; voluntary disclosure.*

Data Availability: *Data are publicly available.*

I. INTRODUCTION

The purpose of this paper is to investigate whether an association exists between legal liability coverage for directors and officers (through D&O liability insurance and the indemnification that a company provides) and the quantity and the quality of the company's voluntary disclosures. Directors and officers can mitigate their personal legal liability through D&O insurance and indemnification, and companies can recoup their indemnification costs through D&O insurance. Core (1997) finds that firms with greater litigation risk are more likely to purchase D&O insurance. Skinner (1994) documents that managers voluntarily disclose bad news,

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fearing litigation risk (against firms, not specifically against directors and officers).[1] Healy and Palepu (2001) argue that voluntary disclosures are credible when adequate penalties such as legal liability exist for disclosing false information. These findings suggest that D&O insurance and indemnification, which lowers directors' and officers' personal legal liability, might also affect voluntary disclosures.

Although D&O insurance is common as a part of the compensation package for U.S. directors and officers, U.S. firms are not required to disclose D&O insurance information. Consequently, I examine Canadian firms on the Toronto Stock Exchange (TSX) between 2000 and 2001 because their insurance data are publicly available. The sample of Canadian firms provides an appropriate setting to test the research questions of this paper, since the D&O insurance and indemnification provisions of the Canada Business Corporation Act (under which my sample firms fall) have been heavily influenced by the American Model Act, and the Canadian provisions and practices are quite similar to those of the U.S. (Schaeftler 1976).[2] However, cross-listed firms must abide by the SEC disclosure rules the same as U.S. firms, while locally listed firms do not have an anti-fraud provision to follow during the sample period.[3] This difference in disclosure rules is likely to have a different impact on the association between liability coverage and disclosure decisions. In other words, liability coverage would have more influence on disclosure decisions of cross-listed firms with high litigation risk than local firms. Thus, the examination using both the cross-listed and the locally listed firms helps to understand how the impact of liability coverage on the disclosures differs between litigation environments in the U.S. and Canada. The finding that a significant association between excess coverage and disclosure decisions appears only in the cross-listed sample confirms my prediction. Since the association between liability coverage and voluntary disclosure is significant even for cross-listed Canadian firms, this research has implications for U.S. firms facing higher litigation risk.

My analysis indicates that an association exists between legal liability coverage and forecast occurrence, frequency, precision, and horizon. In particular, I find that firms with higher excess coverage are less likely to report bad news forecasts for the sample firms that are cross-listed in the U.S., and that the number of bad news forecasts decreases for large cross-listed sample firm having high litigation risk. I also find that higher excess liability coverage leads to disclosures of more precise bad news for the cross-listed sample firms and less timely disclosures of bad news for large cross-listed sample firms.

Tests show that between the two components of excess total coverage, excess D&O insurance and excess cash for indemnification, excess cash is a primary determinant of disclosure decisions for the cross-listed sample firms. Specifically, excess cash is significantly associated with forecast occurrence and frequency of the cross-listed sample. But, both excess D&O insurance and excess cash affect precision decisions of the cross-listed sample firms, and the impact of these two coverage measures is more significant for large cross-listed sample firms having more litigation concerns. As for forecast timing decisions, excess cash is a significant determinant for large cross-listed sample firms. The results, however, do not support my prediction that firms with higher coverage are likely to issue less accurate forecasts, for both the cross-listed and the local sample firms.

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[1] The evidence in Kasznik and Lev (1995) and Baginski et al. (2002) is also consistent with the litigation costs argument.

[2] The recent rule, National Instrument 51-102 *Continuous Disclosure Obligations* (similar to the SEC Rule 10b-5) is likely to affect firms' disclosure behavior during the sample period. In addition to NI 51-102, the Ontario Securities Commission (OSC) has launched the Continuous Disclosure Review Program in July 2000 in order to improve the quality and the timeliness of disclosures. The Program has actually brought several high-profile lawsuits, which would make Canadian firms adjust their disclosure policies even before the NI 51-102 becomes effective.

[3] Ongoing controversy continues in the finance and law area whether foreign firms listed on the U.S. market effectively bond themselves in compliance with U.S. law. Critics argue that such foreign firms face less litigation risk under U.S. law than U.S. firms do, based on the rarity of law enforcement against foreign firms (Siegel 2005). However, according to Coffee (2002), the SEC tends to impose low-visibility sanctions such as warnings or informal contacts before they become public. Accounting studies support the common belief that listing on the U.S. market brings high litigation risk (e.g., Frost and Pownall 1994; Seetharaman et al. 2002). Although the TSX requires firms to follow the regulations for timely disclosure, its enforceability is weak in the sense that a violation of the regulation brings only trading halts.

This paper makes a contribution to the disclosure literature by documenting that liability coverage (as measured by D&O insurance and cash for indemnification) explains, in part, managers' disclosure behaviors. This study also extends the litigation costs argument by examining the relationship between firm-specific legal liability and disclosure decisions. Further, this paper adds to our understanding that liability protection through D&O insurance and indemnification tends to affect managers' disclosure behaviors in an opportunistic way in terms of forecast occurrence, frequency, and horizon, and that indemnification, with a low extent of monitoring authority (such as insurers), has more influence on managers' disclosure behaviors than D&O insurance.

The remainder of the paper is organized as follows. Section II reviews the related literature, and Section III develops hypotheses followed by a description of test designs. Section IV contains sample selection procedures and data descriptions. Section V presents empirical findings and Section VI discusses the robustness checks. Finally, Section VII provides a summary of the findings and discusses directions of future research.

II. LITERATURE REVIEW

Litigation Costs Argument for Voluntary Disclosure

Under SEC Rule 10b-5, shareholders can file lawsuits when managers have made a false statement of a material fact or omitted such a fact. Regarding the omission case, Skinner (1994) argues that managers preemptively disclose bad news to avoid large litigation costs. His rationale is that early disclosure helps prove that managers made timely disclosures of information, and that the early disclosure shortens the nondisclosure period. This in turn allows fewer investors to qualify as members of the plaintiff class, which lowers litigation costs. Skinner (1994) finds evidence that bad news is preempted more frequently than good news. Kasznik and Lev (1995) complement Skinner's (1994) work by showing that firms in bad news periods tend to preempt the announcement of bad news, and that when they do so, they are likely to choose more quantitative and earnings-related disclosures.

Baginski et al. (2002) extend Skinner's (1994) argument by comparing firms' disclosure behaviors in countries with different litigation environments, specifically the U.S. and Canada. They find that Canadian firms in less litigious environments provide greater frequency, higher precision, and longer-term forecasts than U.S. firms. In addition, Baginski et al. (2002) find that Canadian firms tend to issue more forecasts in periods of earnings increase relative to periods of earnings decrease, but have no tendency to issue more forecasts in earnings decrease periods than U.S. firms. Johnson et al. (2001) document a significant increase both in the frequency of forecasts issued by a firm and in the frequency of firms issuing forecasts following the passage of the safe-harbor provision of the 1995 Private Securities Litigation Reform Act (PSLRA). They also report that the increase in disclosure in the post-PSLRA period is more significant for firms with high litigation risk. Francis et al. (1994) provide evidence inconsistent with the litigation costs argument. They fail to find a causal relation between the presence or magnitude of bad news and the incidence of shareholder litigation, indicating that the preemption of bad news may not be effective for preventing litigation. However, a recent paper by Field et al. (2005) argues that an endogenous relation between disclosures and litigation exists, since firms with higher litigation risk are more likely to preempt bad news (preemption effect), while firms that preempt bad news are less likely to be sued (deterrence effect). After controlling for endogeneity using a simultaneous equation approach, Field et al. (2005) show that disclosure has a deterrent effect that bolsters the litigation costs argument.

D&O Liability Insurance

In order to protect directors and officers from personal liability incurred by business decisions and to recruit highly qualified individuals, companies commonly bear the costs of litigation against directors and officers through D&O insurance and indemnification provisions. D&O liability insurance is purchased to recoup costs paid for indemnification or to cover any losses to which indemnification does not apply. In general, D&O insurance policies provide entity coverage that includes both corporate and personal coverage. Specifically, D&O insurance coverage usually (1) reimburses the firm for its indemnification payment for directors and officers, (2) covers individual directors and officers for their wrongful acts to the extent that they have not been indemnified by the firm, or (3) covers the firm to the extent that it is named as defendant along with the directors and officers.[4] Typical D&O policies cover damages, settlements, judgments, and litigation expenses but not civil or criminal fines or penalties, punitive damages, or multiple damages.

Two opposing arguments about D&O insurance have been studied in the literature: the monitoring role and the managerial opportunism argument. I refer to general opportunistic behaviors by managers to mislead investors for their private benefit as managerial opportunism.[5] Proponents argue that D&O insurance plays a governance role, since an insurer thoroughly scrutinizes the ensured, and coverage limits and deductibles exist (Holderness 1990; O'Sullivan 1997). However, opponents argue that D&O insurance lessens the effectiveness of litigation as a monitoring tool. Although the empirical evidence is mixed, recent studies support the latter argument that managerial opportunism is one of the determinants of D&O insurance purchase decisions. Using Canadian data, Core (1997) finds that firms with greater inside voting control tend to purchase more insurance and carry higher coverage. Boyer and Delvaux-Derome (2002) find that firms with weak governance systems, and thus more room for opportunistic behavior, are likely to buy D&O insurance. Chalmers et al. (2002) posit that managers are likely to purchase D&O insurance with high coverage when they access the market with overvalued IPO stocks, since a high probability exists that the directors and officers will be sued by shareholders for any subsequent decline in stock price. Using proprietary information from U.S. firms, Chalmers et al. (2002) find that three-year post-IPO performance is negatively associated with D&O insurance coverage purchased along with the IPO. Overall, prior studies in the literature indicate the managerial opportunism role of D&O insurance.

Although one might expect that the elimination or limitation of legal liability via D&O insurance would result in managerial opportunism, existing evidence does not clearly establish this causal relation, *ex post* managerial opportunism resulting from liability coverage. Nevertheless, previous research suggests that the decision to purchase D&O insurance with high coverage reflects *ex ante* managerial opportunism pertaining to legal liability.

In general, prior studies suggest the following determinants of D&O insurance coverage limits: (1) firm size, (2) leverage, (3) a cross-listing status, (4) governance quality, (5) stock return volatility, and (6) an industry membership. Specifically, Core (2000) predicts that a firm's D&O coverage limit is a function of its governance structure quality and its business risk, since firms with higher business risk—thus greater expected losses—purchase greater limits. Consistent with his predictions, Core (2000) finds that Canadian firms with lower governance quality, greater size, the presence of sales or assets in the U.S., and a cross-listing status in the U.S. markets tend to carry higher coverage limits. Boyer (2005), using Canadian firms, reports consistent results: (1) a negative association between shares of outside blockholders and D&O coverage limits, but (2) a positive association between business risk (as measured by firm size and leverage) and D&O coverage limits. However, Kim (2006), using proprietary data of U.S. firms, reports that firms with greater firm size, higher governance quality, and higher stock return volatility are likely to purchase higher insurance coverage limits. Interestingly, Kim (2006) finds that high-tech firms tend to have lower coverage limits, which is consistent with her argument that high premiums charged to firms with high litigation risk would result in lower coverage limits. Overall, extant studies indicate that firms with higher litigation risk carry higher coverage limits, although the findings are mixed.

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[4] Although the definition of a wrongful act could include a number of activities by directors and officers, it usually refers to any error, misstatement, misleading statement, omission, or neglect, which is partly related to voluntary disclosures.

[5] Timing of voluntary disclosure for CEO stock option awards or earnings manipulation to beat or meet analyst forecasts could be examples of managerial opportunism according to my definition. As defined in Chalmers et al. (2002), "managerial opportunism" is often used in the sense that insiders with superior information sell shares at inflated prices for their own benefit.

Indemnification

In the U.S., indemnification originates from the enactment of the New York General Corporation Law in 1941, which led to the Delaware General Corporation Law in 1943, legislation that influenced the enactment of the federal 1950 Model Business Corporation Act (the Model Act). As a result of a series of amendments over a half century, the current indemnification provisions eliminate liability for negligence, misconduct, or breaches in the duty of care. Most state indemnification statutes have mandatory and permissive provisions. Mandatory indemnification provisions are applied if the director or officer was deemed successful based on the merits in the proceeding's outcome. In those situations where mandatory indemnification cannot be applied, indemnification continues to be permissible, and thus corporations have the power to provide indemnification. Since most lawsuits against directors and officers are settled before trial, firms usually provide indemnifications to directors and officers through charter, by-law, employment contract, or board resolution to the maximum amount allowed by the statutes, if they acted in good faith and in the best interests of the firm. Although the indemnification provision is not limited to disclosures, it is most likely to influence managers' disclosure decisions since the most common lawsuit by stockholders is based on Rule 10b-5 of the 1934 Act (Fischer and Feldman 2002).[6]

Whether the managerial opportunism argument for D&O insurance also holds for indemnification is an open question. One similarity between D&O insurance and indemnification is that indemnification has maximum amounts allowed by the statutes and D&O insurance has coverage limits. The existence of coverage limits (and deductibles) is one rationale for the monitoring role argument.[7] However, compared to D&O insurance, general indemnification provisions tend to be munificent in the sense that exclusion from indemnification occurs only when directors and officers have not acted honestly in good faith and in a manner for the best interests of the firm. But, in practical matters, such exclusion rarely happens, since plaintiffs would intentionally seek not to prove actual intent by bad faith of directors and officers in order to make D&O insurance available for payments to plaintiffs (Black et al. 2006).[8]

Another similarity between D&O insurance and indemnification is that both are subject to the approval of another party; D&O insurance coverage is dependent on an insurer's approval and indemnification is conditional on the approval of a board of directors, a special legal counsel chosen by a board of directors, or shareholders.[9] Since the scrutiny and approval by the insurer is one basis for the monitoring role of D&O insurance, indemnification is expected to have the same monitoring role. However, indemnification can be determined even without the shareholders' approval if there are two or more disinterested directors on a board of directors. In addition, since indemnification can be applied even to a criminal case where D&O insurance is not applicable, the monitoring role of indemnification might be weaker than that of D&O insurance (in other words, its managerial opportunism role might be stronger). Therefore, in this paper, the managerial opportunism argument applies to both D&O insurance and indemnification.

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[6] Under Rule 10b-5, directors and officers can be sued on the grounds that they made a false statement of a material fact or omitted to state it.

[7] I assume that D&O insurance deductibles are currently immaterial, since most D&O carriers have no personal deductible according to a survey by Towers Perrin in 2003.

[8] Black's et al. (2006) discussions only concern outside directors' liability. However, I believe that their argument still applies to all directors' and officers' liability, since typical D&O policies (at least revealed in proxy circulars) treat directors and officers similarly.

Cash Holdings

The theories on corporate cash holdings include (1) the trade-off theory, (2) the financing hierarchy theory, and (3) the agency cost theory. According to the trade-off theory, firms in imperfect capital markets determine the optimal level of cash balances, by trading off the benefits and the costs of cash holding. The benefits of holding cash include the savings on the transaction costs for external financing and opportunity costs of deficits, and economies of scale in cash management. The costs of cash holdings include transaction costs, agency costs, and opportunity costs related to the low return (Opler et al. 1999; Dittmar et al. 2003). Opler et al. (1999) and Dittmar et al. (2003) note that the benefits of holding cash arise from the transaction motive and the precautionary motive; the transaction motive suggests that firms with high costs of external financing and opportunity costs of deficits hold more cash, while the precautionary motive predicts that firms hold more cash to mitigate costs of financial distress, especially when they want to invest in positive NPV projects. On the other hand, according to the financial hierarchy theory and agency cost theory, the level of cash balances is merely the outcome of investment and financing decisions. The financial hierarchy theory based on Myers and Majluf (1984) indicates that firms with high cash flow finance new projects, pay dividends and debts, and accumulate cash, while firms with low cash flow use cash and debts to finance new projects. The agency cost theory suggests that managers with excess cash flow accumulate cash to pursue their own objectives, rather than to maximize shareholders' wealth.

Overall, extant research supports both the trade-off theory and the agency cost theory. Supportive of the trade-off theory, Kim et al. (1998) find empirical evidence that the optimal level of liquid assets is determined by a trade-off between the costs of investing in liquid assets and the benefit of minimizing the need for costly external financing. The extensive study by Opler et al. (1999) also documents that firms with great access to the capital markets hold less cash. Recent studies have focused on the agency cost theory, specifically the association between corporate governance and cash holdings, although the previous studies find weak evidence on the association between agency costs and cash holdings (Opler et al. 1999; Mikkelson and Partch 2003).[10] Dittmar et al. (2003) point out that the weak evidence on agency costs and cash holding may result from using the U.S. data, since the U.S. markets provide strong shareholder protection. Using international data, they find a significantly negative association between corporate governance and cash holdings: firms in countries with poor shareholder protection hold more cash than firms in countries with strong shareholder protection. They also find evidence consistent with the trade-off theory and report that, after controlling for shareholder rights, firms hold more cash when they have good investment opportunities. However, the study by Harford et al. (2008) documents a positive relation between corporate governance and cash holdings. Using extensive measures for corporate governance, they document that U.S. firms with poor governance hold less cash. They conclude that their results are inconsistent with cross-country studies, because country-level variations in shareholder protection dominate firm-level variations in the control of agency problems. After controlling for country-level variations in shareholder protection, Kalcheva and Lins (2007) find that firms with agency conflicts hold more cash, and that this relation is stronger in countries where shareholder protection is weak.

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[9] According to Section 8.55 of the Model Act, the determination is made by one of the following: (1) by the board of directors by a majority vote of all the disinterested directors, if there are two or more disinterested directors, (2) by special legal counsel selected by the board of directors that met the criteria (1), or (3) by shareholders.

[10] Opler et al. (1999) find consistent evidence with their argument that agency costs of managerial discretion are less important for firms with positive NPV projects. Mikkelson and Partch (2003) show no significant differences between the agency costs (as measured by inside ownership and board characteristics) of firms with excess levels of cash holdings and firms with normal levels of cash holdings.

The prior literature suggests the following common determinants of cash holdings: (1) firm size, (2) growth opportunities, (3) cash flows, (4) financial distress, (5) net working capital, and (6) governance quality (e.g., Kim et al. 1998; Opler et al. 1999; Dittmar et al. 2003; Kalcheva and Lins 2007; Harford et al. 2008). In particular, large firms tend to hold lower cash balances, since their cost of external financing is lower than small firms and they have economies of scale in cash management. Firms with growth opportunities tend to hold more cash for future growth and investment. Firms generating higher levels of cash flow tend to hold high levels of cash balances, while firms with financial distress tend to have lower levels of cash balances. Firms with additional liquid assets and good governance quality tend to hold less cash.

III. HYPOTHESIS DEVELOPMENT AND RESEARCH DESIGN

Hypothesis Development

The first hypothesis pertains to the association between excess coverage and the occurrence and frequency of management earnings forecasts. Given the legal liability threat, managers have an incentive to preempt bad news to minimize the expected legal liabilities, since investors can sue them on the grounds of omission of material information. If managers do not have adequate legal liabilities because of having liability coverage, then they are likely to be reluctant to disclose bad news that brings an unfavorable stock response. Thus, managers with high coverage would not disclose bad news forecasts or would disclose fewer bad news forecasts. H1a pertains to forecast occurrence, while H1b pertains to forecast frequency.

H1a: Firms with higher coverage are less likely to issue bad news forecasts.

H1b: Firms with higher coverage are likely to issue fewer bad news forecasts.

Second, I posit that liability coverage affects the decision for timeliness of bad news disclosures. Skinner (1994) provides rationales for why legal liability induces the preemptive disclosures of bad news: (1) managers can make a defensive argument that they disclosed information in a timely manner, and (2) the shorter the nondisclosure period, the smaller the number of purchasers or sellers, which would lead to lower the litigation costs. If managerial opportunism holds, then managers with lower expected legal liabilities might be less concerned about timely disclosure and the length of the nondisclosure period. Therefore, their disclosures of bad news are likely to be less timely, resulting in a shorter time horizon.

H2: Firms with higher coverage are likely to issue less timely bad news forecasts.

The third hypothesis relates to the association between liability coverage and forecast precision. Previous studies suggest that managers facing higher legal liability would issue less precise bad news forecasts. Kasznik and Lev (1995) argue that unless the expectation gap (information asymmetry) is large, managers are less likely to issue quantitative earnings forecasts that could lead to loss of reputation and litigation, whereas in the case of a large expectation gap, managers would have an incentive to issue more quantitative earnings forecasts to narrow a large expectation gap. Bamber and Cheon (1998) point out that the second requirement of SEC Rule 10b-5, related to misleading statements of a material fact, decreases managers' incentive to issue precise earnings forecasts. For instance, range forecasts would turn out to be more accurate than point forecasts, reducing litigation risk. Consistent with their argument, Bamber and Cheon (1998) find that managers with high expected legal liability are likely to provide less precise forecasts. Baginski et al. (2002) also document that Canadian managers tend to issue more precise forecasts than U.S. managers facing higher litigation risk.

In addition, according to Baginski and Hassell (1997), managers' private information increases forecast precision. Baginski and Hassell (1997) and Bamber and Cheon (1998) report that earnings uncertainty reflected in the forecast horizon decreases forecast precision. In other words, early disclosures are likely to be less precise due to uncertainty in private information resulting from early release. Considering H2, that firms with high coverage provide less timely forecasts, bad news forecasts released close to the earnings report date (i.e., less timely disclosures) are likely to be more precise. Taken together, these findings suggest a positive association between liability coverage and forecast precision.

H3: Firms with higher coverage are likely to issue more precise bad news forecasts.

Fourth, I predict that liability coverage affects forecast accuracy. Before the safe harbor provision of the 1995 PSLRA, managers had a duty to update or correct preexisting disclosures.[11] Section 21E of the PSLRA addresses the application of safe harbor: "Nothing in this section shall impose upon any person a duty to update a forward-looking statement." The exact application of this section is, however, unsettled in circuit courts. Some courts still impose a duty to update earnings expectations, but others impose only a duty to correct them (Bain and Johnson 2001). Thus, although responsibility to update is lessened by the safe harbor provision, managers are still likely to be concerned about the duty. Lower expected legal liabilities could cause managers to overlook this requirement, which will result in less accurate forecasts. In addition, more precise forecasts are expected to be less accurate. If H3 holds true, and managers overlook the duty to update expectations because of liability coverage, then firms with higher coverage would issue less accurate bad news forecasts.

H4: Firms with higher coverage are likely to issue less accurate bad news forecasts.

Research Design

Forecast Variables

OCCUR = forecast occurrence; the dummy variable indicating whether a firm issues bad news during the current period; 1 if issued, and 0 otherwise. To find the sign of news, expected earnings are determined in the following order: (1) the median value of most recent forecast(s) on I/B/E/S, (2) existing management forecast if I/B/E/S analyst forecasts are not available, and (3) the seasonal random walk model if I/B/E/S analyst forecasts and existing management forecasts are not available. To avoid the use of stale analyst forecasts or management forecasts, a one-year window prior to a forecast date is searched. A forecast is classified as bad news if a quantitative forecast is lower than the expected earnings or a qualitative forecast has a negative message.

FREQ = forecast frequency; the number of bad news forecasts issued by a firm during a fiscal quarter.

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[11] The duty to update applies to forward-looking information that was true when the forecast was made, while the duty to correct applies to historical facts that were not true when the forecast was made.

HORIZON = forecast horizon. In accordance with previous papers, horizon is determined by the number of calendar days between the forecast date and the fiscal period-end being forecasted (Baginski and Hassell 1997; Bamber and Cheon 1998; Baginski et al. 2002).
PRECISION = forecast precision. A higher value is assigned to the more precise forecasts; *PRECISION* equals 3, 2, 1, and 0 for point, range, open-interval, and qualitative types of forecasts, respectively.
ACCURACY = forecast accuracy; the absolute value of forecast errors. The forecast error is the difference between the most recent management earnings forecasts (as of the report date of actual earnings) and the actual earnings, deflated by share price at the beginning of the forecast quarter.

Liability Coverage Variables

D&O insurance coverage (after subtracting the premium paid to purchase it) is used as a proxy for liability coverage using external money, whereas cash is a proxy for liability coverage using internal money through indemnification. I assume that the total coverage (the sum of D&O coverage and cash for indemnification) captures the extent of the expected legal liability, that is, high coverage proxies for low legal liability. To measure the excess liability coverage (beyond the expected coverage that a firm would carry), I use the residuals from the regression of a coverage measure on its determinants.[12] Excess total liability coverage is measured as the sum of excess D&O insurance coverage and excess cash for indemnification. Each hypothesis has a set of two tests using either (1) excess total coverage or (2) excess D&O insurance and excess cash for indemnification.

EXTOTAL = the sum of excess D&O coverage and excess cash for indemnification.
EXADJCOV = the residual from the regression of adjusted D&O insurance coverage on determinants of D&O insurance coverage limits. Adjusted D&O insurance coverage is the coverage limit minus premium to purchase the limit, scaled by total assets at the beginning of the current quarter. The determinants of adjusted coverage limits include (1) firm size (as measured by the log of total assets), (2) business risk (as measured by a debt ratio), (3) a cross-listing status, (4) governance quality (as measured by the percentage of outside directors on a board of directors and the percentage of shares held by outside blockholders), (5) the volatility of stock returns, (6) membership in high-tech industry, and (7) cash holdings. Cash holdings are included since preliminary tests show that adjusted coverage and cash holdings are positively correlated with a p-value less than 0.01. This significant correlation can be interpreted to mean that firms with more cash tend to purchase D&O insurance with higher coverage, or that firms with legal concerns protect themselves using both D&O insurance and indemnification as a form of self insurance. The regression controls for industry and year fixed effects.[13]
EXCASH = the residual from the regression of cash on determinants of cash holdings. Cash is the sum of cash, cash equivalents, and short-term investments, scaled by total

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[12] To examine the effect of liability coverage level on disclosure decisions, test results using total coverage, D&O coverage, and cash are discussed in the "Robustness Checks" section.

[13] Industry fixed effects are based on Fama and French's (1997) 48-industry groups that tend to have common business risk characteristics.

assets at the beginning of the current quarter. The determinants of cash holdings include (1) firm size (as measured by the log of total assets), (2) growth opportunities (as measured by the market-to-book value ratio and capital expenditures), (3) cash flows (defined as earnings before depreciation and amortization, less interest, taxes, and common dividends), (4) financial distress (as measured by a debt ratio), (5) net working capital (excluding cash), and (6) governance quality (as measured by the percentage of outside directors on a board of directors, and the percentage of shares held by outside blockholders). In addition, a cross-listing status, the volatility of stock returns, a membership in high-tech industry, and adjusted D&O coverage limits are included in the estimation, since excess cash for indemnification can be considered a form of self-insurance. The regression also contains industry and year fixed effects.

Control Variables

The common control variables for tests of forecast properties include firm size, earnings changes, loss, price change, earnings volatility, trading volumes, membership in high-tech industry, membership in regulated industry, and the attributes of corporate governance.

SIZE = the natural log of the firm's total assets at the beginning of the current forecast quarter represents firm size. Cox (1985) provides evidence that management forecasts are associated with firm size.

DECHANGE = 1 if realized earnings in the current period is greater than, or equal to that in the previous period, and 0 otherwise. Baginski et al. (2002) document that firms with decreasing earnings tend to issue forecasts.

LOSS = 1 if a firm reports losses in the period, and 0 otherwise. Ajinkya et al. (2005) document that firms having losses are less likely to provide forecasts.

PCHANGE = beta- and market-adjusted price changes over the preceding 250 trading days: $(P_q - P_{q-4}) - \beta_{250}(INDEX_q - INDEX_{q-4})$.[14] For market adjustment, the S&P 500 index is used for cross-listed firms and the TSE 300 index (currently S&P/TSX composite index) for locally listed firms. According to Verrecchia (1983), firms' willingness to provide voluntary disclosures and what they choose to disclose depends on the market's expectations about the probability distribution over the possibly different values of the manager's private information. The variable is included to control for the firms' "good/bad information" environments when comparing the amount and type of voluntary disclosures. The idea is that the market's expectation of the firms' disclosure possibilities is likely to be reflected in price movements in the period prior to the firms' disclosure.

CVEPS = the coefficient of variation of earnings over the previous three quarters, winsorized at top and bottom 1 percent. The measure of earnings volatility is included, since greater earnings volatility implies a higher likelihood of incorrect forecasts, which, in turn, leads to less willingness to forecast (Waymire 1985).

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[14] Close prices are adjusted for dividends and stock splits.

TRADEVOL = the ratio of the number of shares traded during a fiscal quarter to the number of shares outstanding at the end of quarter. This variable is included to control the effect of market liquidity on disclosure behaviors.

HIGHTECH = 1 if a firm is a member of Pharmaceuticals (SIC codes 2833–2836), R&D Services (8731–8734), Programming (7371–7379), Computers (3570–3577), or Electronics (3600–3674) industries, and 0 otherwise. Investors of high-tech firms would have more demand for management forecasts because those firms tend to be small and risky (Kasznik and Lev 1995; Baginski et al. 2002).

REGULATE = 1 if a firm is a member of Telephone (SIC codes 4812–4813), TV (4833), Cable (4841), Communications (4811–4899), Gas (4922–4924), Electricity (4931), Water (4941), or Financial (6021–6023, 6035–6036, 6141, 6311, 6321, 6331) industries, and 0 otherwise. Firms in regulated industries have less demand for management earnings forecasts, since these firms are required to disclose a great deal of information (Kasznik and Lev 1995; Baginski et al. 2002).

OUTSIDE = the percentage of outside directors on a board of directors. Outside directors are defined as those who are not officers of a firm. Ajinkya et al. (2005) report that firms with greater outside directorship and institutional ownership tend to issue forecasts, and that those forecasts are more precise, more accurate, and less optimistic.

OUTBLOCK = the percentage of shares held by outside blockholders.[15]

Estimation Equations

To examine forecast occurrence for H1a, the ordinary logistic regression models (1) and (2) are estimated. According to H1a, the coefficients of *EXTOTAL*, *EXADJCOV*, and *EXCASH* are expected to be negative ($\alpha_1 < 0$, $\alpha_{1a} < 0$, and $\alpha_{1b} < 0$).

$$\begin{aligned} P(OCURR = 1) = {} & \alpha_0 + \alpha_1 EXTOTAL + \alpha_2 SIZE + \alpha_3 DECHANGE + \alpha_4 LOSS + \alpha_5 PCHANGE \\ & + \alpha_6 CVEPS + \alpha_7 TRADEVOL + \alpha_8 HIGHTECH + \alpha_9 REGULATED \\ & + \alpha_{10} OUTSIDE + \alpha_{11} OUTBLOCK + \varepsilon \end{aligned} \tag{1}$$

$$\begin{aligned} P(OCURR = 1) = {} & \alpha_0 + \alpha_{1a} EXADJCOV + \alpha_{1b} EXCASH + \alpha_2 SIZE + \alpha_3 DECHANGE + \alpha_4 LOSS \\ & + \alpha_5 PCHANGE + \alpha_6 CVEPS + \alpha_7 TRADEVOL + \alpha_8 HIGHTECH \\ & + \alpha_9 REGULATED + \alpha_{10} OUTSIDE + \alpha_{11} OUTBLOCK + \varepsilon \end{aligned} \tag{2}$$

To investigate forecast frequency for H1b, the regression models (3) and (4) are estimated. The coefficients of *EXTOTAL*, *EXADJCOV*, and *EXCASH* are expected to be negative ($\beta_1 < 0$, $\beta_{1a} < 0$, and $\beta_{1b} < 0$).

$$\begin{aligned} FREQ = {} & \beta_0 + \beta_1 EXTOTAL + \beta_2 SIZE + \beta_3 DECHANGE + \beta_4 LOSS + \beta_5 PCHANGE \\ & + \beta_6 CVEPS + \beta_7 TRADEVOL + \beta_8 HIGHTECH + \beta_9 REGULATED + \beta_{10} OUTSIDE \\ & + \beta_{11} OUTBLOCK + \varepsilon \end{aligned} \tag{3}$$

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[15] Canadian firms are required to disclose block shareholders who own more than 10 percent of a firm's stock.

$$\begin{aligned} FREQ = {} & \beta_0 + \beta_{1a}EXADJCOV + \beta_{1b}EXCASH + \beta_2SIZE + \beta_3DECHANGE + \beta_4LOSS \\ & + \beta_5PCHANGE + \beta_6CVEPS + \beta_7TRADEVOL + \beta_8HIGHTECH + \beta_9REGULATED \\ & + \beta_{10}OUTSIDE + \beta_{11}OUTBLOCK + \varepsilon \end{aligned} \tag{4}$$

For H2 regarding forecast horizon, the following regression Equations (5) and (6) are estimated. According to the hypothesis, negative coefficients of excess coverage measures for bad news are expected ($\gamma_1 < 0$, $\gamma_{1a} < 0$, and $\gamma_{1b} < 0$).

$$\begin{aligned} HORIZON = {} & \gamma_0 + \gamma_1EXTOTAL + \gamma_2SIZE + \gamma_3DECHANGE + \gamma_4LOSS + \gamma_5PCHANGE \\ & + \gamma_6CVEPS + \gamma_7TRADEVOL + \gamma_8HIGHTECH + \gamma_9REGULATED \\ & + \gamma_{10}OUTSIDE + \gamma_{11}OUTBLOCK + \gamma_{12}ANNUAL + \varepsilon \end{aligned} \tag{5}$$

$$\begin{aligned} HORIZON = {} & \gamma_0 + \gamma_{1a}EXADJCOV + \gamma_{1b}EXCASH + \gamma_2SIZE + \gamma_3DECHANGE + \gamma_4LOSS \\ & + \gamma_5PCHANGE + \gamma_6CVEPS + \gamma_7TRADEVOL + \gamma_8HIGHTECH \\ & + \gamma_9REGULATED + \gamma_{10}OUTSIDE + \gamma_{11}OUTBLOCK + \gamma_{12}ANNUAL + \varepsilon \end{aligned} \tag{6}$$

To test for H3 regarding forecast precision, an additional control variable of forecast horizon is included. Forecast horizon affects forecast precision because managers have greater uncertainty for longer forecast horizons (King et al. 1990). In accordance with previous studies, forecast horizon is measured by the number of calendar days between the forecast date and the period-end being forecasted. According to H3, the coefficients of excess coverage measures for bad news are expected to be positive ($\delta_1 > 0$, $\delta_{1a} > 0$, and $\delta_{1b} > 0$).

$$\begin{aligned} PRECISION = {} & \delta_0 + \delta_1EXTOTAL + \delta_2SIZE + \delta_3DECHANGE + \delta_4LOSS + \delta_5PCHANGE \\ & + \delta_6CVEPS + \delta_7TRADEVOL + \delta_8HIGHTECH + \delta_9REGULATED \\ & + \delta_{10}OUTSIDE + \delta_{11}OUTBLOCK + \delta_{12}HORIZON + \varepsilon \end{aligned} \tag{7}$$

$$\begin{aligned} PRECISION = {} & \delta_0 + \delta_{1a}EXADJCOV + \delta_{1b}EXCASH + \delta_2SIZE + \delta_3DECHANGE + \delta_4LOSS \\ & + \delta_5PCHANGE + \delta_6CVEPS + \delta_7TRADEVOL + \delta_8HIGHTECH \\ & + \delta_9REGULATED + \delta_{10}OUTSIDE + \delta_{11}OUTBLOCK + \delta_{12}HORIZON + \varepsilon \end{aligned} \tag{8}$$

To test H4 in regard to forecast accuracy, the following Equations (9) and (10) are estimated using point estimates and range estimates.[16] The coefficients of bad news forecasts are expected to be positive ($\theta_1 > 0$, $\theta_{1a} > 0$, and $\theta_{1b} > 0$).

$$\begin{aligned} ACCURACY = {} & \theta_0 + \theta_1EXTOTAL + \theta_2SIZE + \theta_3DECHANGE + \theta_4LOSS + \theta_5PCHANGE \\ & + \theta_6CVEPS + \theta_7TRADEVOL + \theta_8HIGHTECH + \theta_9REGULATED \\ & + \theta_{10}OUTSIDE + \theta_{11}OUTBLOCK + \theta_{12}ANNUAL + \varepsilon \end{aligned} \tag{9}$$

$$\begin{aligned} ACCURACY = {} & \theta_0 + \theta_{1a}EXADJCOV + \theta_{1b}EXCASH + \theta_2SIZE + \theta_3DECHANGE + \theta_4LOSS \\ & + \theta_5PCHANGE + \theta_6CVEPS + \theta_7TRADEVOL + \theta_8HIGHTECH \\ & + \theta_9REGULATED + \theta_{10}OUTSIDE + \theta_{11}OUTBLOCK + \theta_{12}ANNUAL + \varepsilon \end{aligned} \tag{10}$$

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[16] For range estimates, the midpoint of a range is used.

IV. SAMPLE SELECTION AND DATA DESCRIPTION

Sample Selection

The sample consists of Canadian firms listed on the Toronto Stock Exchange (TSX; formerly TSE) from 2000 to 2001.[17] Their D&O insurance data are publicly available in a proxy circular because, since 1993, the Exchange has required firms to disclose firms' risk and their risk management tools in proxy circulars in order to strengthen corporate governance (Core 1997). Specifically, the sample includes (1) Canadian firms that were cross-listed on TSE 300 index (currently S&P/TSX composite index) and U.S. markets, and (2) local firms that were once listed on the TSE 300 index; these are identified through TSE Review (currently TSX eReview) and S&P Research Insight.

Table 1 presents the sample selection process. The initial sample consists of 2,528 cross-listed firm-quarters of 316 firms and 2,984 local firm-quarters of 373 firms identified from the monthly TSE Review and S&P Research Insight. I exclude firms that have undergone a merger or acquisition, have gone bankrupt or reorganized, or were de-listed from TSX. Next, based on the monthly TSE Review, I delete 736 firm-quarters that were not cross-listed during a whole quarter for the cross-listed sample and 949 firm-quarters that were cross-listed at least one month during one quarter for the local sample. I remove firm-quarters whose financials are not available in Compustat and stock prices in CRSP for the cross-listed sample (YAHOO Finance for the local sample). Further, I check firm-quarters for which D&O insurance coverage and premium data are

TABLE 1

Sample Selection

	Cross-Listed Sample		Locally Listed Sample	
Criteria	**No. of Firm-Quarters**	**No. of Firms**	**No. of Firm-Quarters**	**No. of Firms**
TSE Review and S&P Research Insight	2,528	316	2,984	373
M&A, bankrupt, reorganized, or delisted	(576)	(72)	(472)	(59)
Not cross-listed during the whole quarter	(736)	(57)	—	—
Cross-listed at least one month in a quarter	—	—	(949)	(102)
Financials not available in Compustat	(700)	(80)	(937)	(84)
Prices unavailable in CRSP	(91)	(23)	—	—
Prices unavailable in YAHOO Finance	—	—	(111)	(19)
Coverage and premium not available	—	—	—	—
Governance data not available	—	—	—	—
Final sample	425	84	515	109

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[17] The Internet stock bubble burst in the spring of 2000, and high-profile corporate scandals broke in the fall of 2001. To determine whether the market downturn affected high-tech firms' decisions to buy D&O insurance or increase coverage, I examine the changes in coverage level of 18 cross-listed high-tech firms in my sample. Only one of those firms increased the level of coverage. The corporate scandals of 2001 are not likely to have influenced firms' decisions concerning the level of coverage during my sample period between 2000 and 2001 because most of my sample firms could change their insurance contracts in 2002.

not found in a proxy circular available at http://www.sedar.com.[18] Finally, I check the firm-quarters whose governance data (information on outside directors and outside blockholders) are not available in an annual report or a proxy circular. After these deletions and checks, the final sample includes 425 firm-quarters of 84 cross-listed firms and 515 firm-quarters of 109 local firms.

Data Description

Descriptive statistics of liability coverage in Panel A of Table 2 show that the unadjusted D&O insurance coverage for premium paid is significantly greater for the cross-listed sample than for the local sample ($70 million versus $45 million).[19] However, after deflating the coverage limit by a firm's total assets, the difference between the cross-listed sample and the local sample is insignificant. In addition, the cross-listed sample pays a higher premium than the local sample, on average ($0.25 million versus $0.08 million). However, after controlling for firm size, D&O coverage per premium dollar for the cross-listed sample is lower than that for the local sample (1.05 versus 2.18), which implies that D&O insurance is much more expensive for cross-listed firms. This result confirms the common notion that firms cross-listed in U.S. markets face more litigation risk.

Panel A of Table 2 also shows that the cross-listed sample has significantly higher market liquidity as measured by the trading volume, as well as a higher percentage of outside directors and blockholders than the local sample. About 76 percent of cross-listed firm-quarters and 74 percent for local firm-quarters carry D&O insurance, but the difference is not statistically significant.[20] The cross-listed sample includes significantly more firm-quarters having a loss and more high-tech companies than the local sample.

Management earnings forecasts are collected from Factiva (the former Dow Jones News Retrieval Service).[21] Baginski et al. (2002) discuss that the Service is one of preferred means for disclosure by the TSX and covers major news wires and newspapers in Canada. Panel B of Table 2 presents descriptive statistics of bad news forecasts. The statistics forecast occurrence and frequency indicate that the cross-listed firms in a more litigious environment issue more bad news forecasts than the local firms, in line with the litigation costs argument (18 percent versus 8 percent). The cross-listed sample tends to provide more range-type forecasts, while the local sample tends to issue more open-type forecasts. However, forecast horizons for quarterly and annual forecasts are not statistically significant for the cross-listed and the local sample.

V. EMPIRICAL RESULTS

Tests for H1a and H1b: Forecast Occurrence and Forecast Frequency

Table 3 reports estimation results after controlling for other factors that could affect firms' disclosure decisions. Panel A regarding forecast occurrence shows that the coefficient of excess total coverage is significantly negative for the cross-listed firms at the 5 percent significance level,

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[18] The website, http://www.sedar.com, developed by the Canadian Securities Administrators (CSA), provides Canadian public securities filings; SEDAR, which stands for System for Electronic Document Analysis and Retrieval, has been in operation since 1997.

[19] The currency in this paper is Canadian dollars, unless otherwise stated. For firms reporting D&O insurance coverage in U.S. dollars, the average exchange rate over the fiscal year is used to convert U.S. dollars to Canadian dollars. The average exchange rate during the sample period of 2000–2001 was U.S.$0.66 for one Canadian dollar. The rates were not volatile during the sample period, since the changes range from −4.5 percent to +5 percent.

[20] A survey by Towers Perrin (2003) reports that about 92 percent of Canadian survey participants purchase D&O insurance. The percentage of firms carrying D&O insurance in my sample is lower than that in the survey. This could be because I assume that if a firm does not mention D&O insurance in its proxy circular, it does not carry the insurance.

[21] The format of search keywords used is as follows: (anticipat* or believ* or estimat* or expect* or forecast* or predict* or project* or see*) and (earnings or income* or profit* or loss* or net).

TABLE 2
Descriptive statistics

Panel A: Descriptive Statistics for Sample Firm-Quarters

	Cross-Listed Sample (n = 425)			Local Sample (n = 515)			t-tests
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	t-value
ADJCOV	0.07	0.02	0.14	0.08	0.03	0.21	−0.64
CASH	0.16	0.04	0.26	0.11	0.03	0.30	2.58***
TOTAL	0.23	0.09	0.32	0.19	0.08	0.43	1.60
D&O coverage (m$)	70	30	92	45	20	82	4.43***
D&O coverage/*TA*	0.07	0.02	0.14	0.08	0.03	0.21	−0.62
D&O premium (m$)	0.25	0.13	0.31	0.08	0.05	0.12	11.09***
D&O premium/*TA*	0.0004	0.0001	0.0009	0.0002	0.0001	0.0007	4.15***
(D&O coverage/Premium)/*TA*[a]	1.05	0.20	2.91	2.18	0.83	5.22	−3.45***
SIZE	6.95	6.98	2.15	6.56	6.42	1.50	3.29***
PCHANGE	172	53	1,993	43	130	1,577	1.11
CVEPS	0.05	0.20	2.19	0.29	0.30	1.79	−1.86*
TRADEVOL	0.16	0.14	0.14	0.12	0.10	0.08	5.75***
OUTSIDE	80 %	83%	11%	76 %	75%	12%	4.26***
OUTBLOCK	10 %	0%	18%	8 %	0%	13%	2.47**
D&O insurance	No	101 (24%)		No	132 (26%)		0.66
	Yes	324 (76%)		Yes	383 (74%)		
DECHANGE	Down	201 (47%)		Down	237 (46%)		−0.39
	Up	224 (53%)		Up	278 (54%)		
LOSS	No	257 (60%)		No	405 (79%)		6.19***
	Yes	168 (40%)		Yes	110 (21%)		

(continued on next page)

TABLE 2 (continued)

	Cross-Listed Sample (n = 425)			Local Sample (n = 515)			t-tests
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	t-value
HIGHTECH	No	351 (83%)		No	456 (89%)		2.61***
	Yes	74 (17%)		Yes	59 (11%)		
REGULATED	No	386 (91%)		No	481 (93%)		1.47
	Yes	39 (9%)		Yes	34 (7%)		

Panel B: Descriptive Statistics of Bad News Forecasts Data

	Cross-Listed Sample	Local Sample	t-value
Forecast occurrence	75 firm-quarters issued (18%)	42 firm-quarters issued (8%)	4.43***
Forecast frequency:			
Zero forecasts	350 firm-quarters (82%)	473 firm-quarters (92%)	−4.43***
One forecast	64 firm-quarters (14%)	40 firm-quarters (7.7%)	3.16***
Two or more forecasts	15 firm-quarters (5%)	2 firm-quarters (0.3%)	3.62***
Forecast precision:			
Point forecasts	36 (32%)	18 (36%)	−0.44
Range forecasts	53 (48%)	13 (26%)	2.64***
Open forecasts	11 (10%)	12 (24%)	−2.39**
Other type forecasts	11 (10%)	7 (14%)	−0.76
Total	111 (100%)	50 (100%)	

(continued on next page)

TABLE 2 (continued)

Forecast Horizon (in days)	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	t-value
Annual forecasts[b]	265	250	166	218	201	146	1.24
Quarterly forecasts[c]	67	56	87	37	24	58	1.59

***, **, * Significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

[a] The statistics are based on the 324 (383) cross-listed (local) firm-quarters that carry D&O insurance.

[b] The statistics are based on the 56 (25) annual bad news forecasts of the cross-listed (local) sample.

[c] The statistics are based on the 55 (25) quarterly bad news forecasts of the cross-listed (local) sample.

Variable Definitions:

ADJCOV = D&O insurance coverage limit less premium paid, scaled by total assets at the beginning of the current forecast quarter;
CASH = the sum of cash and short-term investments, scaled by total assets at the beginning of the current forecast quarter;
TOTAL = the sum of adjusted D&O insurance coverage and cash, scaled by total assets at the beginning of the current forecast quarter;
SIZE = natural log of total assets at the beginning of the current forecast quarter;
PCHANGE = price change over one year prior to a quarter, adjusted by a firm's beta over 250 trading days and the market index $= (P_q - P_{q-4}) - \beta_{250} * (INDEX_q - INDEX_{q-4})$;
CVEPS = the coefficient of variation of earnings over the previous three quarters;
TRADEVOL = trading volume during a quarter, divided by outstanding volume at the end of quarter;
OUTSIDE = the percentage of outside directors in a board of directors;
OUTBLOCK = the percentage of an outside shareholder who owns more than 10 percent of a firm's stock;
DECHANGE = 1 if earnings of current period are equal to or greater than earnings of previous period, and 0 otherwise;
LOSS = 1 if a firm reports losses in the current period, and 0 otherwise;
HIGHTECH = 1 if a firm is a member of Pharmaceuticals (SIC codes 2833–2836), R&D Services (8731–8734), Programming (7371–7379), Computers (3570–3577), or Electronics (3600–3674) industries, and 0 otherwise; and
REGULATED = 1 if a firm is a member of Telephone (4812–4813), TV (4833), Cable (4841), Communications (4811–4899), Gas (4922–4924), Electricity (4931), Water (4941), or Financial (6021–6023, 6035–6036, 6141, 6311, 6321, 6331) industries, and 0 otherwise.

TABLE 3

Results for H1a and H1b (Forecast Occurrence and Frequency)

Panel A: Logistic Regressions for H1a (Forecast Occurrence)

$$P(OCURR=1)=\alpha_0+\alpha_1 EXTOTAL+\alpha_2 SIZE+\alpha_3 DECHANGE+\alpha_4 LOSS+\alpha_5 PCHANGE+\alpha_6 CVEPS+\alpha_7 TRADEVOL+\alpha_8 HIGHTECH+\alpha_9 REGULATED+\alpha_{10} OUTSIDE+\alpha_{11} OUTBLOCK+\varepsilon \quad (1)$$

$$P(OCURR=1)=\alpha_0+\alpha_{1a} EXADJCOV+\alpha_{1b} EXCASH+\alpha_2 SIZE+\alpha_3 DECHANGE+\alpha_4 LOSS+\alpha_5 PCHANGE+\alpha_6 CVEPS+\alpha_7 TRADEVOL+\alpha_8 HIGHTECH+\alpha_9 REGULATED+\alpha_{10} OUTSIDE+\alpha_{11} OUTBLOCK+\varepsilon \quad (2)$$

	Cross-Listed Sample		Large Cross-Listed Sample		Local Sample	
	Coefficient (1)	**Coefficient (2)**	**Coefficient (1)**	**Coefficient (2)**	**Coefficient (1)**	**Coefficient (2)**
Intercept	−2.62 (0.03)**	−2.52 (0.03)**	−2.66 (0.27)	−2.69 (0.28)	−2.77 (0.03)**	−2.76 (0.03)**
EXTOTAL	−2.65 (0.04)**		−5.02 (0.04)**		0.07 (0.94)	
EXADJCOV		−1.40 (0.54)		−5.22 (0.34)		−0.68 (0.63)
EXCASH		−3.00 (0.03)**		−4.96 (0.07)*		0.60 (0.52)
SIZE	0.39 (0.00)***	0.37 (0.00)***	0.18 (0.55)	0.18 (0.56)	−0.06 (0.68)	−0.05 (0.73)
DECHANGE	−1.06 (0.00)***	−1.05 (0.00)***	−0.94 (0.03)**	−0.94 (0.03)**	−0.89 (0.02)**	−0.89 (0.02)**
LOSS	−0.16 (0.66)	−0.15 (0.67)	0.13 (0.78)	0.12 (0.80)	−0.21 (0.65)	−0.20 (0.66)
PCHANGE	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.02)**	0.00 (0.02)**	0.00 (0.18)	0.00 (0.16)
CVEPS	0.22 (0.00)***	0.23 (0.00)***	0.10 (0.38)	0.10 (0.39)	−0.00 (0.97)	−0.01 (0.96)
TRADEVOL	1.72 (0.10)*	1.78 (0.09)*	1.74 (0.18)	1.73 (0.19)	3.56 (0.06)*	3.53 (0.06)*
HIGHTECH	0.20 (0.67)	0.22 (0.63)	−0.10 (0.91)	−0.09 (0.91)	−2.34 (0.04)**	−2.36 (0.04)**
REGULATED	−1.38 (0.02)**	−1.24 (0.05)**	−1.39 (0.03)**	−1.42 (0.10)	−0.29 (0.67)	−0.24 (0.72)

(continued on next page)

TABLE 3 (continued)

	Cross-Listed Sample		Large Cross-Listed Sample		Local Sample	
	Coefficient (1)	**Coefficient (2)**	**Coefficient (1)**	**Coefficient (2)**	**Coefficient (1)**	**Coefficient (2)**
OUTSIDE	−2.07 (0.12)	−2.04 (0.13)	0.26 (0.91)	0.26 (0.91)	1.25 (0.44)	1.13 (0.48)
OUTBLOCK	−1.47 (0.15)	−1.46 (0.15)	−4.31 (0.04)**	−4.32 (0.04)**	−1.94 (0.24)	−1.96 (0.24)
n	425	425	213	213	515	515
χ^2 (p-value)	83.39 (0.00)	83.77 (0.00)	49.27 (0.00)	49.27 (0.00)	22.19 (0.02)	23.02 (0.03)

Panel B: Multiple Regressions for H1b (Forecast Frequency)

$$FREQ = \beta_0 + \beta_1 EXTOTAL + \beta_2 SIZE + \beta_3 DECHANGE + \beta_4 LOSS + \beta_5 PCHANGE + \beta_6 CVEPS + \beta_7 TRADEVOL + \beta_8 HIGHTECH + \beta_9 REGULATED + \beta_{10} OUTSIDE + \beta_{11} OUTBLOCK + \varepsilon \quad (3)$$

$$FREQ = \beta_0 + \beta_{1a} EXADJCOV + \beta_{1b} EXCASH + \beta_2 SIZE + \beta_3 DECHANGE + \beta_4 LOSS + \beta_5 PCHANGE + \beta_6 CVEPS + \beta_7 TRADEVOL + \beta_8 HIGHTECH + \beta_9 REGULATED + \beta_{10} OUTSIDE + \beta_{11} OUTBLOCK + \varepsilon \quad (4)$$

	Cross-Listed Firms		Large Cross-Listed Firms		Local Firms	
	Coefficient (3)	**Coefficient (4)**	**Coefficient (3)**	**Coefficient (4)**	**Coefficient (3)**	**Coefficient (4)**
Intercept	0.08 (0.34)	0.10 (0.29)	−0.16 (0.36)	−0.09 (0.43)	0.03 (0.38)	0.03 (0.38)
EXTOTAL	−0.13 (0.19)		−1.11 (0.01)***		−0.02 (0.37)	
EXADJCOV		0.05 (0.40)		−0.71 (0.23)		−0.05 (0.25)
EXCASH		−0.23 (0.09)*		−1.21 (0.01)***		−0.00 (0.50)

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TABLE 3 (continued)

	Cross-Listed Firms		Large Cross-Listed Firms		Local Firms	
	Coefficient (3)	**Coefficient (4)**	**Coefficient (3)**	**Coefficient (4)**	**Coefficient (3)**	**Coefficient (4)**
SIZE	0.05 (0.00)***	0.05 (0.00)***	0.08 (0.05)*	0.07 (0.10)*	−0.00 (0.44)	−0.00 (0.43)
DECHANGE	−0.15 (0.00)***	−0.15 (0.00)***	−0.19 (0.00)***	−0.19 (0.00)**	−0.08 (0.00)***	−0.08 (0.00)***
LOSS	0.02 (0.33)	0.02 (0.33)	0.04 (0.33)	0.05 (0.29)	−0.01 (0.41)	−0.01 (0.43)
PCHANGE	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.15)	0.00 (0.15)
CVEPS	0.04 (0.00)***	0.04 (0.00)***	0.02 (0.12)	0.02 (0.11)	−0.00 (0.49)	−0.00 (0.48)
TRADEVOL	0.13 (0.24)	0.15 (0.21)	0.11 (0.34)	0.14 (0.31)	0.37 (0.01)**	0.36 (0.02)**
HIGHTECH	0.10 (0.07)*	0.11 (0.05)*	0.13 (0.21)	0.12 (0.24)	−0.11 (0.01)***	−0.11 (0.01)***
REGULATED	−0.20 (0.01)**	−0.18 (0.02)**	−0.25 (0.01)***	−0.21 (0.07)*	−0.03 (0.26)	−0.03 (0.28)
OUTSIDE	−0.19 (0.18)	−0.21 (0.15)	−0.16 (0.32)	−0.16 (0.31)	0.12 (0.17)	0.12 (0.17)
OUTBLOCK	−0.26 (0.02)**	−0.27 (0.02)**	−0.36 (0.04)**	−0.35 (0.05)**	−0.11 (0.16)	−0.11 (0.16)
n	425	425	213	213	515	515
Adjusted R^2	0.16	0.16	0.18	0.18	0.02	0.02

***, **, * Represent the significance at the 1 percent, 5 percent, and 10 percent levels, respectively. The p-values in parentheses in Panel B are based on one-tailed tests.

Variable Definitions:

OCCUR = 1 if one or more bad news forecasts are issued during a quarter, and 0 otherwise;
FREQ = the number of forecasts issued by a firm during a quarter;
EXADJCOV = the residual from the regression of adjusted D&O insurance coverage on determinants of D&O insurance;
EXCASH = the residual from the regression of cash on determinants of cash holdings;
EXTOTAL = the sum of excess D&O insurance coverage and excess cash;
SIZE = natural log of total assets at the beginning of the current forecast quarter;
DECHANGE = 1 if earnings of current period are equal to or greater than earnings of previous period, and 0 otherwise;

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while the coefficient of the local sample is not significant. The tests regarding forecast frequency provide similar results; the coefficient of excess total coverage for the cross-listed firms is negative, but not significant (Panel B of Table 3). These results indicate that cross-listed firms in more litigious environments behave opportunistically by not issuing bad news forecasts. But liability coverage does not necessarily precipitate similar behaviors in local firms in less litigious environments.

When excess total coverage is split into excess D&O coverage (*EXADJCOV*) and excess cash for indemnification (*EXCASH*), only the coefficient of *EXCASH*, α_{1b}, is significantly negative for the full cross-listed sample (Panel A of Table 3). Panel B of Table 3 for forecast frequency shows consistent results; only excess cash is a significant determinant of forecast frequency decisions.

I investigate further the large firms' behaviors by dividing the full sample into the large and the small subsamples, since large firms tend to be sued more often than small firms. Panels A and B of Table 3 for the large cross-listed sample show that managers with higher total coverage tend to provide fewer bad news forecasts, and that only excess cash appears to significantly affect disclosure occurrence and frequency decisions of large firms for bad news forecasts. As for the small cross-listed sample, the coefficients of three excess coverage measures are negative, but not significant (not tabulated). The results imply that for large cross-listed firms with more legal concerns, excess cash for indemnification (internal money for liability protection) is more important to managers' decisions than D&O insurance coverage (external money for liability protection). This finding is consistent with the prediction that indemnification and D&O insurance do not affect disclosure decisions in the same way. This result is also intuitive, since indemnification is allowed even in criminal cases as long as managers have acted in good faith for the best interests of the firm, while typical D&O insurance requires scrutiny for payments and excludes claims due to fraud by the ensured or those associated with regulatory agencies.

Table 3 also indicates that firm size, earnings changes, managers' information environment, earnings volatility, and a membership in regulated industry are associated with forecast occurrence and frequency decisions for the cross-listed sample. As for the local sample, earnings changes, market liquidity, and a membership in high-tech industry are statistically significant.

Tests for H2: Forecast Horizon

The test sample for H2 and H3 consists of 111 bad news forecasts by the cross-listed sample and 50 bad news forecasts by the local sample. Table 4 shows that the coefficient of excess total coverage for bad news, γ_1, is negative but insignificant for the cross-listed sample. However, when the full cross-listed sample is divided into two groups of small and large firms, large cross-listed firms having higher liability coverage tend to issue bad news in a less timely manner. When excess D&O insurance coverage and excess cash are used, only excess cash for indemnification is significantly negative for large cross-listed firms, which is consistent with forecast occurrence and frequency hypothesis.

Tests for H3: Forecast Precision

The significantly positive coefficients, δ_1, δ_{1a}, and δ_{1b}, in Table 5 indicate that excess total coverage, excess D&O coverage, and excess cash for indemnification increase the forecast precision of bad news for the cross-listed sample. The results indicate that the significance of excess total coverage for the cross-listed sample is driven by both excess D&O insurance coverage and excess cash. Furthermore, when the samples are divided into two groups of small and large firms, excess D&O insurance and excess cash of the large cross-listed firms is also more significantly positive at the 5 percent significance level. Stated differently, when a large firm has higher D&O

TABLE 4 (continued)

	Cross-Listed Firms		Large Cross-Listed Firms		Local Firms	
	Coefficient (5)	**Coefficient (6)**	**Coefficient (5)**	**Coefficient (6)**	**Coefficient (5)**	**Coefficient (6)**
OUTBLOCK	−148.11 (0.09)*	−173.79 (0.06)*	−193.26 (0.28)	−331.66 (0.19)	−95.51 (0.34)	−53.52 (0.42)
ANNUAL	174.75 (0.00)***	170.33 (0.00)***	155.18 (0.00)***	165.58 (0.00)***	221.60 (0.00)***	216.69 (0.00)***
n	111	111	55	55	50	50
Adjusted R^2	0.36	0.36	0.55	0.54	0.44	0.43

***, **, * Represent the significance at the 1 percent, 5 percent, and 10 percent levels, respectively. The p-values in parentheses are based on one-tailed tests.

Variable Definitions:
HORIZON = the number of calendar days from the forecast date to the fiscal period-end being forecasted;
EXADJCOV = the residual from the regression of adjusted D&O insurance coverage on determinants of D&O insurance;
EXCASH = the residual from the regression of cash on determinants of cash holdings;
EXTOTAL = the sum of excess D&O insurance coverage and excess cash;
SIZE = natural log of total assets at the beginning of the current forecast quarter;
DECHANGE = 1 if earnings of current period are equal to or greater than earnings of previous period, and 0 otherwise;
LOSS = 1 if a firm reports losses in the current period, and 0 otherwise;
PCHANGE = price change over one year prior to a quarter, adjusted by a firm's beta over 250 trading days and the market *index* = $(P_q - P_{q-4}) - \beta_{250} *(INDEX_q - INDEX_{q-4})$;
CVEPS = the coefficient of variation of earnings over the previous three quarters;
TRADEVOL = trading volume during a quarter divided by outstanding volume at the end of quarter;
HIGHTECH = 1 if a firm is a member of Pharmaceuticals (SIC codes 2833–2836), R&D Services (8731–8734), Programming (7371–7379), Computers (3570–3577), or Electronics (3600–3674) industries, and 0 otherwise;
REGULATED = 1 if a firm is a member of Telephone (4812–4813), TV (4833), Cable (4841), Communications (4811–4899), Gas (4922–4924), Electricity (4931), Water (4941), or Financial (6021–6023, 6035–6036, 6141, 6311, 6321, 6331) industries, and 0 otherwise;
OUTSIDE = the percentage of outside directors in a board of directors;
OUTBLOCK = the percentage of an outside shareholder who owns more than 10 percent of a firm's stock; and
ANNUAL = 1 if a forecast is annual, and 0 otherwise.

TABLE 5
Results for H3 (Forecast Precision)

$$PRECISION = \delta_0 + \delta_1 EXTOTAL + \delta_2 SIZE + \delta_3 DECHANGE + \delta_4 LOSS + \delta_5 PCHANGE + \delta_6 CVEPS + \delta_7 TRADEVOL + \delta_8 HIGHTECH + \delta_9 REGULATED + \delta_{10} OUTSIDE + \delta_{11} OUTBLOCK + \delta_{12} HORIZON + \varepsilon \quad (7)$$

$$PRECISION = \delta_0 + \delta_{1a} EXADJCOV + \delta_{1b} EXCASH + \delta_2 SIZE + \delta_3 DECHANGE + \delta_4 LOSS + \delta_5 PCHANGE + \delta_6 CVEPS + \delta_7 TRADEVOL + \delta_8 HIGHTECH + \delta_9 REGULATED + \delta_{10} OUTSIDE + \delta_{11} OUTBLOCK + \delta_{12} HORIZON + \varepsilon \quad (8)$$

	Cross-Listed Firms		Large Cross-Listed Firms		Local Firms	
	Coefficient (7)	**Coefficient (8)**	**Coefficient (7)**	**Coefficient (8)**	**Coefficient (7)**	**Coefficient (8)**
EXTOTAL	5.50 (0.02)**		17.16 (0.02)**		4.39 (0.25)	
EXADJCOV		5.30 (0.09)*		60.94 (0.03)**		19.64 (0.00)***
EXCASH		5.77 (0.08)*		14.90 (0.04)**		6.27 (0.11)
SIZE	−0.23 (0.15)	−0.23 (0.16)	−0.70 (0.16)	−1.44 (0.03)**	0.04 (0.89)	0.00 (0.99)
DECHANGE	1.08 (0.04)**	1.09 (0.05)**	1.41 (0.13)	0.78 (0.45)	0.74 (0.27)	1.26 (0.09)*
LOSS	1.50 (0.00)***	1.50 (0.00)***	1.10 (0.12)	1.34 (0.06)*	0.99 (0.27)	2.10 (0.05)**
PCHANGE	0.00 (0.03)**	0.00 (0.03)**	−0.00 (0.97)	0.00 (0.47)	−0.00 (0.23)	−0.00 (0.51)
CVEPS	0.00 (0.95)	0.00 (0.96)	0.01 (0.93)	0.04 (0.78)	0.57 (0.11)	0.70 (0.05)**
TRADEVOL	0.41 (0.87)	0.35 (0.89)	−7.27 (0.28)	−1.75 (0.82)	−6.44 (0.11)	−5.25 (0.21)
HIGHTECH	−2.69 (0.00)***	−2.69 (0.00)***	0.17 (0.94)	−1.85 (0.45)	1.71 (0.40)	2.94 (0.16)
REGULATED	−1.16 (0.25)	−1.16 (0.25)	0.07 (0.96)	1.53 (0.34)	−0.22 (0.87)	−0.30 (0.83)
OUTSIDE	3.08 (0.08)*	3.08 (0.08)*	6.77 (0.05)*	8.38 (0.02)**	4.10 (0.11)	6.30 (0.03)**
OUTBLOCK	1.33 (0.44)	1.37 (0.43)	12.68 (0.06)*	17.96 (0.02)**	−6.35 (0.12)	−10.55 (0.02)**

(continued on next page)

TABLE 5 (continued)

	Cross-Listed Firms		Large Cross-Listed Firms		Local Firms	
	Coefficient (7)	**Coefficient (8)**	**Coefficient (7)**	**Coefficient (8)**	**Coefficient (7)**	**Coefficient (8)**
HORIZON	0.00 (0.72)	0.00 (0.70)	0.00 (0.99)	−0.00 (0.74)	−0.00 (0.40)	−0.00 (0.79)
n	111	111	55	55	50	50
χ^2 (p-valuc)	43.66 (0.00)	43.67 (0.00)	24.00 (0.02)	27.21 (0.01)	12.07 (0.44)	18.39 (0.14)

***, **, * Represent the significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Variable Definitions:

PRECISION = 3 (2, 1, 0) for point (range, open, other) forecasts;

EXADJCOV = the residual from the regression of adjusted D&O insurance coverage on determinants of D&O insurance;

EXCASH = the residual from the regression of the sum of cash on determinants of cash holdings;

EXTOTAL = the sum of excess D&O insurance coverage and excess cash;

SIZE = natural log of total assets at the beginning of the current forecast quarter;

DECHANGE = 1 if earnings of current period are equal to or greater than earnings of previous period, and 0 otherwise;

LOSS = 1 if a firm reports losses in the current period, and 0 otherwise;

PCHANGE = price change over one year prior to a quarter, adjusted by a firm's beta over 250 trading days and the market $Index = (P_q - P_{q-4}) - \beta_{250} * (INDEX_q - INDEX_{q-4})$;

CVEPS = the coefficient of variation of earnings over the previous three quarters;

TRADEVOL = trading volume during a quarter, divided by outstanding volume at the end of quarter;

HIGHTECH = 1 if a firm is a member of Pharmaceuticals (SIC codes 2833–2836), R&D Services (8731–8734), Programming (7371–7379), Computers (3570–3577), or Electronics (3600–3674) industries, and 0 otherwise;

REGULATED = 1 if a firm is a member of Telephone (4812–4813), TV (4833), Cable (4841), Communications (4811–4899), Gas (4922–4924), Electricity (4931), Water (4941), or Financial (6021–6023, 6035–6036, 6141, 6311, 6321, 6331) industries, and 0 otherwise;

OUTSIDE = percentage of outside directors in a board of directors;

OUTBLOCK = percentage of an outside shareholder who owns more than 10 percent of a firm's stock; and

HORIZON = the number of calendar days from the forecast date to the fiscal period-end being forecasted.

coverage and more cash, its bad news tends to be more precise, indicating that the liability protection through D&O insurance and indemnification also affects precision decisions of firms having high litigation risk.[22]

The positive association between liability coverage and forecast precision corroborates the findings by Ajinkya et al. (2005) and Karamanou and Vafeas (2005). Ajinkya et al. (2005) find no significant association between governance quality (as measured by outside directors and institutional investors) and forecast precision. They discuss directors' fear of greater litigation exposure resulting from more precise forecasts, as a possible explanation for the insignificant association. Using a broad set of governance variables, Karamanou and Vafeas (2005) report a negative association between governance quality and forecast precision. That is, forecast precision decreases with more effective governance quality. They provide the following possible explanations: (1) that well-governed firms are more concerned with their obligation not to mislead shareholders, and (2) that those firms make additional disclosures of bad news at the expense of precision (to mitigate risks resulting from additional disclosures). Those two studies provide a common possible explanation that managers' legal concerns lead to a sacrifice of forecast precision. The finding that, after controlling for governance quality, bad news forecasts issued by firms with excess coverage tend to be more precise implies that those firms do not necessarily sacrifice precision of forecasts, since their legal liability concerns are already mitigated by liability coverage.

However, the implication of the positive association between liability coverage and forecast precision for managerial opportunism is not definitely conclusive. If more precise forecasts are also more biased, then the decision to provide a more precise forecast by managers with high liability coverage can be viewed as more opportunistic. On the other hand, if more precise forecasts are not biased, then the positive association can imply that high coverage does not necessarily lead to an opportunistic choice of forecast precision.[23]

Although tests for the local sample show similar results, the model fit with p-values higher than the 10 percent significance level reveals that the independent variables do not adequately explain forecast precision of the local sample firms. Table 5 also indicates the negative effects of a membership in high-tech industry, but the positive effects of earnings changes, the presence of loss, managers' information environment, and outside directors on the board on forecast precision.

Tests for H4: Forecast Accuracy

After the truncation of extreme outliers in the top 5 percent, the most recent 39 point- and range-type bad news forecasts from the cross-listed sample and 18 bad news forecasts from the local sample are available for accuracy tests. No significant difference in forecast accuracy appears between the cross-listed and the local sample, and the test results indicate that forecast accuracy is not associated with liability coverage (not reported in table). However, insignificant results may be attributable to the small sample size and the measurement error problem due to the translation of

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[22] The same tests are performed only on firms that carry D&O insurance for the following reasons: (1) such firms are likely to be concerned with litigation risk, or (2) as assumed, the lack of a firm's D&O insurance disclosure in its proxy circular means that the firm does not carry D&O insurance, but this assumption can be problematic. Nevertheless, the tests strongly show that the higher liability protection cross-listed firms have, the more precise bad news they release.

[23] I find no statistically significant association between liability coverage and forecast biases, indicating that more precise forecasts due to low legal liability (through high coverage) are not likely to be more biased. However, the insignificant results may be due to small sample size and measurement errors arising from translation of U.S. dollars into Canadian dollars (as discussed in the test results of forecast accuracy).

U.S. dollars into Canadian dollars.[24] Future research using a larger sample consisting of firms that report in U.S. dollars will provide a more conclusive answer as to whether liability coverage is a significant determinant of forecast accuracy.

VI. ROBUSTNESS CHECKS

Measurement of Liability Coverage

I examine whether the overall level measures of coverage have the same effect on managers' disclosure behaviors as excess coverage measures do. As for H1a and H1b, the test results using the overall level of the three measures are qualitatively similar to the main findings using excess coverage measures. Specifically, the coefficients of total coverage are negative but statistically insignificant; the coefficients of cash, however, are significantly negative at the 5 percent significance level (not tabulated). These results are consistent with the finding that forecast occurrence and frequency decisions are mainly driven by excess cash for indemnification. In regard to the forecast horizon of the cross-listed sample, all three of the coverage measures are consistently negative, but insignificant. As for forecast precision, all three of the measures are not significant and only the coefficients of D&O insurance are positive (not tabulated). The tests for forecast accuracy are consistent with the findings in Section V; all three of the liability coverage measures are statistically insignificant.

Measurement of Firm Size

Total assets are used as a proxy for firm size, since they are more stable than market value of equity during the sample period of 2000 and 2001. To examine whether test results differ when using an alternative proxy for firm size, I conduct tests using the market value of equity. Test results for the hypotheses are similar to the main findings using total assets; all three measures of coverage have signs with predicted directions, although some coefficients lose significance.

Interdependence of Sample Firm-Quarters

To the extent that different firm-quarters of the same firm are not independent, this autocorrelation problem can lead to dubious t-statistics for forecast frequency (reported in Panel B of Table 3). To address the problem, I conduct the additional tests as follows: (1) the Durbin and Watson (1950; hereafter, Durbin-Watson) test for the presence of autocorrelation in the residuals, and (2) the Newey and West (1987; hereafter, Newey-West) estimation that provides autocorrelation- and heteroscedasticity-consistent standard errors. First, the Durbin-Watson for tests using (1) excess total coverage (*EXTOTAL*), and (2) excess D&O insurance (*EXADJCOV*) and excess cash (*EXCASH*) are close to the value of two that indicates no autocorrelation. Second, tests based on the Newey-West autocorrelation- and heteroscedasticity-consistent standard errors provide results consistent with the main findings. For the full cross-listed sample, the coefficients of excess total coverage and excess cash are significantly negative at the 5 percent significance level (not reported in table). For the large cross-listed sample, the results are similar: the coefficients of excess total coverage and excess cash are significantly negative at the 10 percent and the 5 percent level, respectively, while the coefficient of excess D&O insurance coverage is negative, but insignificant. The coefficients of all three measures for the small cross-listed firms, however, are not significant.

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[24] Although the exchange rates were not volatile during the sample period, the choice of exchange rates could have affected the power of a test. Forecast errors deflated by share price, mostly in decimals between −0.1 and 0.1, could be sensitive to the choice of exchange rate.

Managers' Risk Aversion

A classic economic theory indicates that an insurer would not offer full insurance, even in a competitive market, leaving the ensured an incentive to take proper care. D&O insurance and indemnification are not likely to shift fully a managers' risk, since it has exclusion clauses and coverage limits. Risk-averse managers still have to choose an optimal disclosure strategy that can protect their liability risk from lawsuits, given the same level of liability coverage, since their actions affect the outcome liability. Moreover, one could expect that risk aversion would affect both managers' choice of liability coverage and disclosures, for given firm size and litigation risk and, thus, the test results would differ. For instance, a risk-averse manager would protect himself excessively by increasing the coverage level and/or disclosing more bad news earlier. If this case is true, then the association between coverage and the likelihood of issuing a bad news forecast would be positive, and thus managers' risk aversion could weaken my findings.

VII. SUMMARY AND CONCLUSIONS

I examine whether an association exists between liability coverage and forecast occurrence, frequency, horizon, precision, and accuracy. Using Canadian firms whose D&O insurance data are publicly available, I find that an association exists between excess liability coverage and disclosure decisions (except forecast accuracy) of the cross-listed but not the local sample firms. In particular, firms with higher excess coverage are less likely to provide bad news forecasts for the sample firms that are cross-listed in U.S., and that the number of bad news forecasts decreases for the large cross-listed sample facing higher litigation risk.[25] High liability coverage also leads to less timely disclosures of bad news for large cross-listed sample firms. In addition, managers with higher liability coverage tend to disclose more precise bad news for the cross-listed sample firms. These results confirm the findings by previous studies in which legal liability can partly explain firms' voluntary disclosure decisions, especially for bad news (Skinner 1994; Kasznik and Lev 1995; Baginski et al. 2002).

In addition, I find that between the two components of excess total coverage, excess D&O insurance and excess cash for indemnification, excess cash is a primary determinant of disclosure decisions for the cross-listed sample firms. Specifically, excess cash is significantly associated with forecast occurrence and frequency of the cross-listed sample, and with forecast timing decisions of the large cross-listed sample firms facing more litigation concerns. But, both excess D&O insurance and excess cash affect the precision decisions of the cross-listed sample firms, and the impact of these two coverage measures is more significant for large cross-listed sample firms having more litigation concerns. These results confirm the argument that firms are concerned with greater litigation risk resulting from more precise forecasts. The results do not support, however, the prediction that firms with higher coverage are likely to issue less accurate forecasts, for both the cross-listed and the local sample firms.

This paper has the following limitations and future research opportunities. First, excess cash might not be a direct proxy for excess litigation coverage. The significance of excess cash for the cross-listed firms' disclosure decisions, but insignificance for the local firms' decisions strengthens the validation of excess cash as a proxy for litigation coverage. However, to the extent excess cash (as measured after controlling for the known determinants of corporate cash holdings) captures other attributes that are not controlled but are related to attributes of management forecasts, the findings of this research could be weak. Second, the main findings of this paper arise from Canadian firms cross-listed in the U.S. and from a limited sample period. The examination of U.S. firms that are entirely subject to U.S. law and regulations will add to our understanding of the association between liability coverage and managerial decisions. Further, recent corporate scandals have changed the shape and environment of corporate governance in the U. S., such as the establishment of the Sarbanes-Oxley Act of 2002 (SOX). Whether legal liability coverage has the same effect on managers' disclosure decisions even in the post-SOX period is an empirical question. A re-examination of my findings using U.S. data in the pre- and post-SOX periods will provide insight to this question.

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[25] The decision to disclose could increase the level of liability coverage. If endogeneity exists, then we would observe changes in the level of coverage before changes in disclosure frequency. My sample shows that only four cross-listed firms have increased D&O insurance coverage limits and then increased disclosure frequencies. In addition, my sample firms' D&O insurance coverage limits are quite stable over the sample period. These observations indicate that my findings are not affected by endogeneity.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editors note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Management, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

YURI BIONDI, ARNALDO CANZIANI, and THIERRY KIRAT (editors), *The Firm as an Entity: Implications for Economics, Accounting and the Law* (London, U.K.: Routledge, 2007, ISBN 978-0-415-41443-2, pp. xii, 387).

The Firm as an Entity: Implications for Economics, Accounting and the Law is a combination of 11 original articles, including articles written by the editors, and seven reprints of important classics. The collective and ambitious objective is to argue that the theory of the firm should be revised to view the firm as an independent entity in its own right, the objective of which is to ensure the continuity of the firm by generating stakeholder payments that allow the stakeholders (employees, suppliers, and shareholders, seen only as input suppliers) to "satisfice" in a boundedly rational environment. A firm as an entity is defined by Biondi (Chapter 2) as "a *dynamic system of interactions, interdependencies, complementarities*. These relationships, located in time and space, are not merely (nor fully) contractual nor bargained: their dynamic system is different in nature from any static equilibrium of prices or nexus of contracts, property rights included" (italics in the original, p. 6).

This book has a number of other interesting attributes. First, it criticizes current economic theories of the firm as missing out on many of the real-world aspects of the firm. Thus, the neoclassical theory of the firm that sees the firm purely as a nexus of equilibrium prices where the firm is a "black box," which, with a given technology, optimizes with these prices is rejected as abstracting from the character of real-world firms. Similarly, the view that sees the firm as only a nexus of contracts concerning property rights (Jensen and Meckling), which builds on Coase's fundamental perspective that firms exist because they can handle some transactions more efficiently than the market, is seen as flawed. This approach is regarded as inadequate because of the inability of contracts to deal with complementaries between assets and with intellectual capital, and as the firm as an entity cannot really be said to exist at all. This also means that derivative theories of the contractual view—transaction cost theory and agency theory—fail to provide a holistic view of the firm. In contrast, the authors have much more sympathy with the Berle and Means argument that the firm is comprised of different groups with differing interests and that attention must be paid to their interrelationships. The second additional attribute of the book is that it sees accounting as one of the two foundations of the firm-accounting and the contractual system.

The accounting system provides a dynamic picture of economic results for the entity of the firm over time, provides values for items not priced by the market, and monitors performance over time.

The next interesting attribute is that the new articles in the book are mainly written by continental European authors, with a Franco-Italian basis (five and three authors, respectively). However, except for the chapter by Canziani (Chapter 8) on early European theories of the firm, the analysis is strongly Anglo-Saxon, apart from a few references here and there and a favorable view of European stakeholder aspects of corporate governance. This is disappointing. Surely, there is more recent and germane European literature. However, the book gives a very good and comprehensive review, albeit of a critical nature, of most economic and some legal theories of the firm and how these theories may be affected by entertaining an entity perspective of the firm.

In some ways the idea of including reprints is a good one for teaching purposes. Here, reprints are used either to show that some of the points made in the book have been anticipated earlier or to provide foundational arguments for the entity concept of the firm. However, it may be that some teachers would prefer to prescribe their own reading.

The book has four parts that develop the argument for seeking an entity theory of the firm. Part I consists of two chapters comprising a brief introduction by the editors and an overview of the book by Biondi (Chapter 2). Part II is entitled "On the economic theory of the firm as an institution and an organisation." The first chapter (Chapter 3) by Weinstein is a very good article that looks critically at the current state of the theory of the firm. It is worth reviewing this article in more depth than some of the other articles, as he makes a strong case for the entity approach. He rejects the neoclassical approach, with its dominance of equilibrium prices. He identifies two modern approaches, one stemming from Berle and Means, with their recognition of the interests of different groups within the firm and that management and shareholders have differing interests. He argues that their analysis requires the firm to be regarded as an entity. The other approach is the contractual approach referred to above, where the firm is seen purely as a nexus of contracts that is regarded as insufficient to capture all of the characteristics of the firm. This leads him to urge the resource-based view of the firm and to suggest a unified theory of the firm, combining its legal, managerial, technological, and accounting characteristics.

The remainder of Part II comprises four reprints, which either support the entity theory—Simon (Chapter 4) and Berle (Chapter 7)—or show the generally neglected importance of accounting to the theory of the firm—Shubik (Chapter 5) and Coase (Chapter 6). Simon and Berle argue from a market and organization perspective that the assumptions of competition and profit maximization are unrealistic in the complexities of the real world. A theme taken up again by Anthony in a well-known article in Part III (Chapter 12) where he urges the substitution of achieving a satisfactory return as the business objective rather than profit maximization, which he also argues favors the wealthy. Shubik indicates 16 limitations of the general equilibrium model of price determination for the study of the firm, while recognizing its importance in achieving efficient production and distribution. He sees accounting as attempting to provide an information system in the environment faced by firms and calls for a reconciliation between accounting and economic sciences. Coase's article indicates that he and others addressed this challenge in the 1930s by recognizing that it is accountants who calculate costs internal to the firm and that these calculations must be consistent with the economists' view of costs. Much remains to be done to achieve this integration, but it is aided by the entity theory's acceptance of accounting as one pillar of the theory of the firm with the legal system being the second.

Part III is titled "Perspectives for accounting, law and economics: Lessons from the past." The aim of this part is to show earlier concerns for treating the firm as an entity. It starts with a scholarly chapter by Canziani, mainly on German and Italian accounting theory for the firm in the early 20th century (Chapter 8). He synthesizes the famous work of German researchers and an

Italian scholar to provide a unified theory of the firm, encompassing law, accounting, and economics, where production and income are regarded as the dual of each other and both are seen to be dynamic. In the next chapter, Kirat similarly looks at the work of four other pioneer thinkers, two French and two from the United States. This early work raises three points. First, the firm is seen as an institution subject to the rules of law and, second, the firm is in a dynamic environment and, third, this literature attempts to integrate these views. In Chapter 10, Avi-Yonah and Sivan look at the history of the legal approaches to the firm. They identify three legal views of the firm as either as an aggregate of the participants or as a constructed legal person with the firm as a real entity. This latter view is bolstered by a second reprint from Berle (Chapter 11), where it is argued that there is a need for the legal concepts of the firm to recognize the firm's reality. Chapter 13 is a reprint of an article by Stauss, who synthesizes a number of theories and expands the concept of the "going concern," which leads to advocating the firm as a real entity.

Part IV is entitled "Essays on economic, legal and accounting features of the firm as an entity" and explores how adopting an entity perspective affects a number of disciplines—accounting, the law, finance, and corporate governance, and also seeks to provide building blocks for the theory. In looking at accounting, Biondi (Chapter 14) first revisits some of the earlier points and reiterates and reinforces the earlier criticisms of extant theories of the firm. He sees the firm "as a managed and organised *system of relationships* not only of the contractual or bargaining sort" (italics in the original, p. 241). Within this framework, he sees accounting as giving a view of what he calls the "special economics" of the firm, that is, income after meeting all costs, including the cost of equity, in a dynamic way over time. This income is seen as available to all stakeholders. Here, accounting represents the ongoing process of the firm using the entity concept, showing its results as they emerge and providing a method of regulating and organizing the whole firm. Although he recognizes that financial accounting systems are currently controversial, he seems to advocate using the traditional financial accounting system as best fitting the entity theory.

Gindis (Chapter 15) again revisits previous theories and provides building blocks for the entity theory. Here, the firm is seen as a distinguishable entity separate from its participants, which is cohesive and durable over time. Against this, one might consider the view that the firm's boundaries are becoming less clear where complex supply chains exist. Manfrin (Chapter 16) considers how regulation would be affected by taking an entity perspective. Marzo (Chapter 17) follows the seeming convention of the book and first criticizes extant finance theory. More importantly, he attempts to sketch some of the possible changes in finance theory generated by taking an entity perspective. These include the abandonment of shareholder wealth maximization in favor of fair compensation for all stakeholders in existing imperfect, incomplete markets populated by actors with limited rationality. He also urges the resurrection of the unsystematic risk as an important element for the firm to manage and the need to create financial slack to help achieve the firm's goal of continuity and therefore the rebirth of optimal capital structure. These are far-reaching changes.

Finally, Moore and Rebérioux in Chapter 18 consider how the entity view of the firm may impact corporate governance. Again, the history of corporate governance is considered and existing theory is criticized. They argue that the current concern about the divergence of interests between shareholders and management is misplaced. Rather, the concern should be about the divergence of interest between a coalition of management and influential shareholders as against the interests of other stakeholders. They suggest that an entity approach to corporate governance requires that the current United States model be augmented with the European social model so as to act as a check on the above dominant interest.

The original articles contained in this volume are scholarly contributions written by authors in full command of their material. Especially welcome is the recognition that accounting systems can play a role in the theory of the firm. Reading the criticisms of current theories of the firm, it is clear that a more comprehensive, empirically consistent and multi-disciplinary approach is required, and thus the objective of the book is achieved. This is, I suspect, preaching to the converted, although the proponents of rival theories may be less easy to convert. However, experience suggests that it is not easy to incorporate new firm attributes into theory.

Moreover, there is a tendency in the book, often successfully, to show that current theory is flawed and then to claim that this suggests the need for an entity theory of the firm. This is, of course, is a big leap. This criticism is probably unfair, given that the authors do not claim to have a fully fledged theory, but further research is clearly necessary. For example, the entity theory abandons shareholder wealth maximization in favor of ensuring the continuity of the firm and the payment of satisficing returns to stakeholders. It is not clear what would be the outcomes in markets comprised of satisficing firms. Moreover, such firms may also not be efficient and are likely to turn down what would be conventionally profitable opportunities. Again, it may be unfair, but there is a strong need in all parts of the book at least to sketch how the various aspects of the theory might be made operational.

The book is technical and at a sophisticated level, and the more general reader may find that some items are not explained sufficiently. It would also help if all the ramifications of the firm viewed as an entity were explained early and in one place. Understandably, for a collection of articles, there is a fair amount of repetition between some of the chapters. The book can be recommended strongly to researchers and post-graduate students interested in the theory of the firm. It should also appeal to accountants interested in the role of accounting systems within the theory of the firm and similarly to lawyers concerned about the part that contracts play in this area.

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FRANK CLARKE and GRAEME DEAN, *Indecent Disclosure: Gilding the Corporate Lily* (Cambridge, U.K.: Cambridge University Press, 2007, ISBN-13 978-0-521-70183-9, pp. xxi, 274).

Indecent Disclosure: Gilding the Corporate Lily is described by the authors as advocating the "...'Sydney School' of accounting's push for reform of the law of company accounts, first proposed by Chambers in his 1973 *Securities and Obscurities* monograph..." (p. xx). The book leads on from the authors' two editions of *Corporate Collapse* (Clarke et al. 1997, 2003) and other works (for example, Clarke and Dean 1992; Clarke et al. 2004; Dean et al. 2008) to address four main themes, summarized in the following extract:

> First, that financial disclosure in accord with conventional accounting generally fails to disclose a company's wealth and progress and that the newly heralded IFRSs will do little to remedy that;
> second, that the corporate governance regimes currently in place are misplaced and do not offer remedies for the corporate ills evident in history and repeated in the recent past;
> third, that auditors' near impossible task is alleviated neither by the prospect of companies' compliance with the IFRSs nor by the implementation of the general features of the corporate governance regimes;
> fourth, and most importantly, though it runs counter to the thinking underpinning the current corporate governance regimes, there is increasing evidence that the prevailing corporate structure needs radical modification to cope with, and be governable in, the modern business environment (pp. 12–13).

While it is generally accepted that financial statements should show a true and fair view of the financial position and performance of an entity, the authors' view is that at the present time "...virtually nothing in the public domain is as misleading as companies' financial statements" (p. 106). It is argued that this state is not the result of dishonesty on the part of accountants but rather the rules that they are constrained to apply. "Accountants are victims rather than villains" (p. 106). The accounting profession is charged with having pursued standardization of input and processing without regard for the output of accounting. Financial statement items prepared under the current financial reporting standards are claimed to lack *serviceability*, that is, the items are unable to be used to derive "...reliable indicators of an entity's wealth, financial performance, rate of return, earnings per share, debt to equity, solvency, asset backing, and the like" (p. 95).

The authors describe the current system of accounting as being expenditure capitalization with an impairment override coupled with matching. The system produces financial statements that include numbers that are no more than "...artifacts of the processing rules imposed upon accountants through the compulsory imposition of Accounting Standards. Some of the data are pure fiction. They are not merely unrepresentative of what they describe, they have nothing to describe—no real-world referent" (p. 66). Deferred tax assets and liabilities and goodwill are discussed as examples of such items. The authors do not see the adoption of IFRS as providing hope for improvement in the serviceability of accounting data, as "...the existing system is patched up by the addition of some partial fair-value adjustments for certain assets and liabilities" (p. 76) and will remain "...constrained by the conventional historic-cost thinking, like matching" (p. 214).

The authors view the corporate problems of recent years as having strong parallels to earlier times, and, while they are generally accepting of the various governance reforms of recent years, they are pessimistic as to effectiveness. The reforms are basically viewed as being more of the same kind of change introduced in response to earlier problems and do not address the flawed accounting model. Similarly for the reforms introduced and proposed for the conduct of audits. The reforms address the process of auditing with particular emphasis on promotion of independence. Auditing is criticized for focusing on compliance with financial reporting standards and, given the authors' view that the standards simply reflect the flawed accounting model, audits inevitably fails to address the truth and fairness of the financial statements in respect of the underlying economic reality. The position of the auditor is summarized as follows: "Auditors continue to be in a no-win situation. The count is over before they enter the ring" (p. 77).

The authors describe in some detail the potential for intra-group shuffling of funds within corporate group structures to defeat the interests of creditors, employees, and other parties, and they are strongly critical of consolidation accounting. Although a corporate group does not have separate legal capacity, the consolidated financial statements present the position and results of the parent company and its subsidiary companies and other entities as if the set were a single entity.

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REINER QUICK, STUART TURLEY, and MARLEEN WILLEKENS (editors), *Auditing Trust and Governance: Developing Regulation in Europe* (London, U.K.: Routledge, 2008, ISBN 13: 978-0-415-44889-5 (hbk), pp. xvii, 283).

This book has 13 chapters. Two are overview essays that examine the concept of a "trust crisis" in Europe in the aftermath of major accounting scandals (Chapter 1) and explore audit regulation in its global context (Chapter 13). Another chapter attempts to explain the transition of audit regulation in the United States from what the author describes as "*laissez faire* to governmental control." The remaining ten chapters summarize regulatory developments in auditing in Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Spain, the U.K., and Russia. All 13 essays are previously unpublished, and the authors and editors are all academics. The intended audience is educators, students, researchers, regulators, and professionals working in the fields of auditing, accountancy, and corporate governance.

The three editors are described as having edited the book on behalf of the European Auditing Research Network (EARNet)—an organization formed in April 2000 to foster research and exchange ideas among auditing scholars and researchers at a European level. The purpose of the book is to analyze the response to the high-profile accounting scandals, including Enron, Parmalat, Ahold, and WorldCom, as it has developed in Europe and to review how regulation has been revised to restore confidence in the contribution of auditing to corporate governance. Although accounting scandals were the motivating force for significant changes in regulation, none of the accounting scandals in particular countries are explained in any detail. Enough of them are mentioned for particular European countries, however, to put to rest the notion that they are exclusively a phenomenon of the United States.

The first chapter, "In the Name of Trust: Some Thoughts About Trust, Audit Quality, and Audit Regulation in Europe," examines whether a trust crisis was really present in Europe and was sufficient to warrant the wave of regulation that started in 2002 and was largely in place throughout Europe by 2006. The focus of the chapter is primarily on the research on audit quality differentiation in Europe and potential related research topics. The chapter appropriately questions how differences in the European environment should influence research design, but it does not set the stage for the following chapters on individual countries. It is an interesting stand-alone essay but not a framing mechanism for what follows.

The final chapter, "Understanding Regulation in its Global Context," briefly summarizes the overarching provisions of the revised EU Eighth Directive on audit regulation throughout Europe, explains existing and well-known theories of regulation in relation to European developments, and extols the virtues of academics as critics of regulation. The role envisioned for auditing researchers is exemplified in the following excerpts:

> It is essential to retain a degree of scepticism or inquisitiveness as to the ability of regulators to cast light on, and improve, audit practice. How efficient and effective are contemporary regulatory practices? Are regulations working? ...Where is the revised Eighth Directive having an impact? Will Europe be able to operate a regulatory system that is distinctive in practical implementation from the U.S. Sarbanes-Oxley regime...? (p. 271)

The questions developed in this chapter might have provided a framework for the chapters on individual countries, but those chapters are for the most part not directed toward answering such questions.

The chapters on individual European countries provide some information on each of the key topics of auditor education, training, and entry requirements; approach to audit standard setting; ethics and independence requirements; auditor liability; oversight of audit regulation; and corporate governance. The coverage of these topics, however, is not uniform among the chapters. Across the chapters, the topics are not presented in the same order, and the depth of coverage of particular topics varies considerably. The chapter on the United States is an oddity in this collection focused on Europe. It covers major developments in the history of 70 years of regulation in the United States in serviceable, broad strokes, but gets many of the details wrong.

The chapters on individual countries provide a high-level introduction to the practice of auditing in those countries, but the auditing scholar in search of insight on cross-country comparisons may be frustrated at the lack of detail on some key issues. On the issue of rotation of audit firms, for example, Spain and Italy have had mandatory rotation requirements. In Spain, the reader is informed that a requirement that auditors could not be engaged by the same organization for a period of less than three and more than nine years, existed from 1988 to 1995, but it was eliminated largely as a result of lobbying by audit firms (pp. 187, 196–97). In Italy, there was a similar requirement, but when Parmalat was required to rotate audit firms, after rotation, the former auditor, Grant Thornton, continued to audit a subsidiary, Bonlat, which was used to perpetrate significant aspects of the fraud (p. 145). There is no information of the effectiveness of the requirement in Spain during its seven-year existence or why the implementation of the rule was ineffective in Italy. Another issue that is not even identified is the effect of control of the audit-standard-setting process by practicing auditors. When practicing auditors control the standards to which they will be held accountable, there is a conflict of interest. On the other hand, practicing auditors generally possess a knowledge advantage on the subject matter of auditing; so they cannot be excluded from the process. The book encourages auditing scholars to be skeptical about the ability of regulators to be effective in this arena, but it is almost unquestioning toward the acceptance of control of the standard-setting process by practicing auditors. There is no skepticism, for example, about various approaches to involving people who are not practicing auditors in the process or oversight of the process. Also, there is no exploration of how the conflict-of-interest versus knowledge-advantage factors are balanced and how effective the balancing is. If the non-practicing auditor members can be retired partners from major firms or defendants' lawyers who exclusively represent auditing firms, then practicing auditors can still maintain control of the process. While each chapter explains the approach to audit standard setting in the country, the degree to which the process is or was effectively controlled by practicing auditors and the result of that dominance is not subjected to careful scrutiny. Nevertheless, the book is a worthwhile resource for auditing scholars seeking an introduction to auditing regulation in European countries, and each chapter includes an extensive list of references for more detailed study.

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CAPSULE COMMENTARY

By the Book Review Editor

GEOFF BURROWS, *Promise Fulfilled: The History of the Accounting Discipline at the University of Melbourne 1925–2004* (Carlton, Vic., Australia: Melbourne University Publishing, 2006, ISBN 978 0 52285 272 1, pp. xv, 280).

The richness of the long tradition of accounting research and educational leadership at the University of Melbourne is well brought out in this engrossing history written by Geoff Burrows, himself part of that tradition. In conducting his research, he corresponded with, or interviewed, numerous individuals associated with accounting at Melbourne in order to develop a great many vignettes that testify so well to the warmth and humanity of the colleagues, families and friends about whom he writes.

This is not only an interesting recitation over eight decades of the comings and goings and achievements of the accounting faculty, as well as developments in the curriculum and degree programs, but it is in addition a series of overlapping biographical sketches of the principals. It is, above all, a well-researched study of the cultural evolution of this important department.

Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended May 31, 2007. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s).*

	Number of Manuscripts	Percent	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	39	8.32	39	8.32
31 ≤ Days ≤ 60	153	32.62	192	40.94
61 ≤ Days ≤ 90	153	32.62	345	73.56
91 ≤ Days ≤ 120	86	18.34	431	91.90
121 ≤ Days	38	8.10	469	100.00
Total	469	100.00		

The mean review time was 70; the median review time was 66 days.

New Submissions by calendar year (January 1 to December 31)

1992	241
1993	234
1994	231
1995	195
1996	230
1997	195
1998	196
1999	239
2000	260
2001	260*
2002	324
2003	327
2004	307
2005	389
2006	413
2007	443

*2001—in addition to the 260 regular submissions, 68 MSs were submitted to the *TAR* Quality of Earnings Conference.

The acceptance rate (defined as number of MSs accepted divided by the number of new submissions) over the past 5 years is between 12–16% per year.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and editors are responsible for providing constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

FORMAT

1. All manuscripts should be formatted in 12-point font on 8 1/2 x 11" paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we".
5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years*. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a cross-sectional equation; re-form*. See *Webster's* for correct usage.

Keywords: The abstract must be followed by at least three keywords to assist in indexing the paper and identifying qualified reviewers.

Data availability: A line immediately following the Keyword identifiers should indicate whether the data are available (see "Data Availability" later in this statement).

ABSTRACT/INTRODUCTION

An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Within-text citations are made using an author-year format. Cited works must correspond to the list of works listed in the "References" section. Authors should make an effort to include the relevant page numbers in the within-text citations.

1. In the text, works are cited as follows: author's last name and year, without comma, in parentheses. For example: one author, (Berry 2003); two authors, (Fehr and Schmidt 2003); three or more authors, (Dechow et al. 1998); more than one work cited, (Cole and Yakushiji 1984; Dechow et al. 1995; Levitt 1998); with two works by the same author(s), (Nelson 2003, 2005).
2. Unless confusion would result, do not use "p." or "pp." before page numbers. For example, (Dechow and Dichev 2002, 41–42).
3. For cited works that include more than one work by an author (or same co-authors) that are published in the same year, the suffix a, b, etc., is to follow the date in the within-text citations and in the "References" section. For example: (Baiman and Rajan 2002a, 2002b).
4. When the author's name is mentioned in the text, it need not be repeated in the citation. For example: "Cohen et al. (2005) provide ..."
5. Citations to institutional works should use acronyms or short titles where practicable. For example: (NCFFR, The Treadway Commission 1987).
6. If the paper refers to statutes, legal treatises, or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a "References" section that contains only those works cited within the text. Each entry should contain all information necessary for unambiguous identification of the published work. Use the following formats (which follow *The Chicago Manual of Style*):

1. Arrange citations in *alphabetical order* according to the surname of the first author or the name of the institution or body responsible for the published work.
2. Arrange citations in *chronological order* when two or more works are by the same first author (regardless of co-authors). Two or more works by the *same author(s)* published in the same year are distinguished by letters a, b, etc., after the year.
3. Use authors' initials instead of proper names.

4. For two or more authors, separate authors with a comma, including a comma before "and" (Dechow, P. M., R. Sloan, and A. Sweeney).
5. Date of publication follows the author's (authors') name(s).
6. Titles of journals or newspapers should not be abbreviated.

Sample entries are as follows:

Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.

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615 STANFORD UNIVERSITY Graduate School of Business is seeking qualified applicants for full-time positions, starting in the 2009–2010 academic year. The successful candidates are expected to contribute innovative and impactful research related to business, energy, and the natural environment, to teach in the school's M.B.A. program and its Ph.D. program. All ranks and relevant disciplines will be considered. Junior applicants should have or expect to complete a Ph.D. before September 1, 2009. Senior applicants are expected to have an excellent record of research and teaching. Applications will be reviewed on a rolling basis during the Autumn quarter. Interviews will begin in September 2008, and therefore applicants, particularly senior applicants are strongly encouraged to submit their applications as soon as possible. For efficiency, accuracy, and speed, applicants are strongly encouraged to submit their applications electronically by visiting the website: http://www.gsb.stanford.edu/recruiting and uploading their *curriculum vitae*, research papers and publications, and teaching evaluations (if applicable) to that site. Alternatively, you may also email electronic materials to: Faculty_Recruiter@GSB.Stanford.Edu; or mail (non-returnable) materials in soft or hard copy to: Faculty Recruiting Manager, Box EE, Graduate School of Business, Stanford University, Stanford, CA 94305–5015. Please allow three weeks for any non-web processing. Applicants should have three letters of recommendation emailed to the preceding address. The application deadline is January 12, 2009. Stanford University is an Equal Opportunity Employer and is committed to increasing the diversity of its faculty. It welcomes nominations of and applications from women and minority groups, as well as others who would bring additional dimensions to the University's research, teaching and clinical missions.

616 MONTANA STATE UNIVERSITY seeks applicants to fill one tenure-track position in Accounting for fall 2009. Montana State University has an enrollment of approximately 12,000 students and is located in Bozeman, an attractive, culturally alive small city, 90 miles north of Yellowstone National Park. Fully accredited by the AACSB, the College of Business offers a Bachelor of Science in Business with options in Accounting, Finance, Management, and Marketing, and a Master of Professional Accountancy. Colleagues in all areas of accounting will be considered, however, our greatest teaching needs are in the areas of tax, accounting information systems, governmental accounting, managerial, and financial accounting. In addition, research activities and service consistent with the College's mission are expected. The position requires a Ph.D. or D.B.A. in Accounting or a closely related field from an AACSB-accredited university. For more information, please visit: http://www.montana.edu/msuinfo/jobs/faculty/; or contact Anne Christensen at: Email: annec@montana.edu; Phone: (406) 994-2043. ADA/AA/EO/Veteran's Preference.

617 NORTH DAKOTA STATE UNIVERSITY College of Business (AACSB) invites applications for a tenure-track position in the Department of Accounting and Information Systems effective August 15, 2009. Rank is open depending on qualifications. Preferred area is financial accounting but other accounting areas will be considered. Minimum qualifications: Ph.D. or D.B.A. in accounting or related area with research and teaching potential, (ABD will be considered), good interpersonal skills, demonstrated effective written and verbal communication skills. If the degree is in a related area, the applicant must provide evidence of his/her ability to teach accounting courses and accomplish accounting-related research. Preferred qualifications: proven record of publications and teaching. Application procedure: screening begins August 1, 2008 and continues until filled. Send application letter, *vita*, teaching evaluations, evidence of scholarly effort, and name, address, phone number of at least three references to: Dr. Donna Dietz, Search Committee Chair, PO Box 5075, NDSU, Fargo, ND 58105; or email: donna.dietz@ndsu.edu. NDSU is an Equal Opportunity institution. For more information: http:/www.ndsu.edu/ndsu/jobs/non_broadbanded/index.shtml.

618 IOWA STATE UNIVERSITY Department of Accounting invites applications for a tenure-track appointment to begin Fall 2009. Applications for Assistant, Associate, or Professor will be considered in all areas of accounting. Tax preferred. Applicants should possess the commitment and ability to be productive scholars and excellent teachers. Minimum requirements include an earned doctorate in accounting or a closely related field prior to beginning the appointment. An established research program and a history of successful teaching experiences will be expected at the Associate or Professor levels. Review of applications will begin on October 31, 2007 and will continue until the position is filled. Submit a letter of application, *curriculum vitae* (include email address), a statement of teaching and research interests, and names of three references to: Dr. Marvin Bouillon, Iowa State University, Department of Accounting, 2330 Gerdin Business Building, Ames, Iowa 50011–1350; or Email: isuaccounting@iastate.edu. ISU is an EO/AA Employer.

619 DUKE UNIVERSITY Fuqua School of Business is seeking to fill one tenure-track faculty position in accounting for the 2009/2010 academic year. Candidates should have a Ph.D. or be close to completing that degree, excellent research skills, and the ability and interest to teach managerial accounting. Hiring decisions will be based on the candidate's ability to contribute to the research and teaching missions of the Fuqua School of Business. Materials from doctoral students should be received by January 15, 2009; others should send their materials as soon as possible. Interested individuals should complete the electronic application at: http://faculty.fuqua.duke.edu/areas/accounting/accounting_recruiting.html. Duke University is an Equal Opportunity/Affirmative Action Employer, and is committed to fostering a diverse educational environment and encourages applications from members of groups under-represented in academia.

620 UNIVERSITY OF SOUTH ALABAMA Accounting Department invites applications for a tenure-track position to teach undergraduate and graduate courses in accounting beginning Fall 2009. Applicants at all ranks will be considered. Applicants must possess a Ph.D. (or D.B.A) with a concentration in accounting or be ABD. Qualifications should include a primary teaching interest in auditing, professional certification (CPA and/or CIA), and an interest in professional service. The successful candidate should demonstrate excellence in teaching and a commitment to intellectual contributions. Applicants must submit a letter of application, current *curriculum vita*, and names (with phone numbers) of three references to: Dr. Russell Hardin, Chairperson, Department of Accounting, Mitchell College of Business, 307 University Blvd, Mobile, AL 36688. Electronic applications may be sent to: rhardin@usouthal.edu. Review of applications will begin October 1, 2008 and continue until the position is filled. The University of South Alabama is an Equal Opportunity/Equal Access Employer.

621 UNIVERSITY OF MISSOURI Columbia School of Accountancy is seeking applications for a tenure-track faculty position at the Assistant Professor level for Fall 2009. Candidates for the position must be able to teach Financial Accounting or Auditing, and commit to high-quality scholarly research. Applications are encouraged from experienced Assistant Professors as well as new Ph.D.s or those nearing completion of their Ph.D. Salary and research support are competitive. Please send letter of interest, *vita*, three reference letters, evidence of teaching competence, and a sample of working papers to: Dr. Vairam Arunachalam, Director, School of Accountancy, University of Missouri, 303-B Cornell Hall, Columbia MO 65211. Screening will begin September 15, 2008, and continue until the position is filled. The University of Missouri is an Equal Opportunity/Affirmative Action Employer. Applications from minority and women candidates are strongly encouraged for this position. To request ADA accommodations, please contact the University's ADA Coordinator at (573) 884–7278 (V/TTY).

622 UNIVERSITY OF NORTHERN IOWA Department of Accounting invites applicants for a tenure-track position as Assistant or Associate Professor of Accounting available August 2009. Applicants should have a primary teaching interest in Managerial/Cost Accounting with a secondary interest in at least one other area of Accounting. Required qualifications include a Ph.D./D.B.A. from an AACSB-accredited institution (ABDs will be considered with evidence of degree completion by August 1, 2009), a record of excellence in teaching, a demonstrated ability to produce publishable research, evidence of strong communication and interpersonal skills. Professional certification is preferred. For full description go to: http://www.cba.uni.edu/dbweb/pages/about/faculty-rec-detail.cfm?Department=Accounting. Send current *vita* and letter of application to: Dr. Mary Christ, Chair, Accounting Search Committee, University of Northern Iowa, 354 Curris Business Building, Cedar Falls, IA 50614–0127; Phone: (319) 273–2108; Fax: (319) 273–2922. Complete applications received by September 15, 2008 will be given full consideration. The Department encourages applications from minority persons, women, persons with disabilities, and veterans. The University is an Equal Opportunity Employer with a comprehensive plan for affirmative action. The University is a smoke-free campus. Email: Mary.Christ@uni.edu; or visit the website.

623 KAPLAN UNIVERSITY is a thriving division of Kaplan, Inc., a subsidiary of The Washington Post Co., and one of the nation's leading providers of educational and career services. We are seeking talented instructors with a passion for education, and a desire to share their knowledge and skills teaching day and evening courses. You'll use your experience and background to present well-prepared, and clear lectures to help students excel. This will involve formal classroom instruction, exams, student evaluation, and consulting on necessary equipment and educational material. Successful candidates must possess: •Master's degree in accounting •Practical experience and dedication •Prior teaching or corporate training experience relevant to your subject area. As a Kaplan University instructor, you will have the unique opportunity to contribute to, and be a part of, an innovative learning environment. Please apply online at: www.kaplan.edu/careers. EOE.

624 UNIVERSITY OF CALIFORNIA, RIVERSIDE Anderson Graduate School of Management invites applications for tenure-track faculty positions in Accounting. We are interested in hiring scholars at the Associate/Full Professor level. We seek candidates with superior research training who have achieved or have the potential to achieve outstanding scholarly distinction in their area of research, and who can provide senior leadership. Demonstrated success in the instruction of demanding M.B.A. and executive students is also important. File review will begin 10/1/2008 and will continue until position has been filled. Interested candidates should send, preferably by email with attachments (e.g., PDF), a detailed letter of interest, *curriculum vitae*, a recent selected publication or working paper, evidence of teaching excellence, and the names of four-six individuals as confidential references to: Leo Gonzalez, leo.gonzalez@ucr.edu. Candidates must have the Ph.D. degree. For more information go to http://www.agsm.ucr.edu/. UCR is an Affirmative Action/Equal Opportunity Employer.

625 KENNESAW STATE UNIVERSITY (KSU) invites nominations and applications for the Dean of the Coles College of Business. KSU is a rapidly growing and progressive university in Georgia's public system of higher education. Located on an attractive campus in suburban Atlanta, KSU currently enrolls over 21,000 traditional and nontraditional baccalaureate, masters, and doctoral students. The Coles College of Business has an outstanding national and international reputation as a leading business school. Recently ranked "among the best" by CEO magazine, the College is AACSB International accredited and has 126 full-time faculty members, over 50 supporting faculty and 30 staff in the School of Accountancy and the Departments of Economics and Finance, Management and Entrepreneurship, Leadership and Executive Development, and Marketing and Professional Sales. The School of Accountancy has separate AACSB International accreditation. The Coles College offers an undergraduate B.B.A. degree with majors in accounting, finance, economics, international business, marketing, management, and professional sales, as well as an M.B.A. and Master's of Accounting degree. In addition, the Executive M.B.A. is a top-ranked program for experienced professionals. In 2009, KSU will begin offering an innovative, executive D.B.A. degree for working professionals. Review of applications will begin immediately, and continue until the position is filled. It is preferred, however, that all nominations and applications be submitted to the search firm prior to December 29, 2008. For a complete position description, refer to Current Opportunities at: http://www.parkersearch.com, and for additional information on KSU, refer to: http://www.kennesaw.edu. Laurie C. Wilder, Senior Vice President, Katie Bain, Principal, Parker Executive Search, (770) 804-1996, ext. 108; kbain@parkersearch.com. KSU is an Affirmative Action/Equal Opportunity Employer.

626 UNIVERSITY OF SOUTH CAROLINA School of Accounting invites applications for a tenure-track position open at all ranks beginning in Fall 2009. Candidates should have a primary teaching interest in Managerial Accounting, Financial Accounting, or Financial Statement Analysis, and must possess a doctorate or expect to complete their doctorate by August 2009. In addition to being committed to excellent teaching, applicants must be ambitious and motivated researchers. Candidates with a research interest in the area of international accounting or sustainable enterprise are particularly encouraged to apply. Salaries are competitive and commensurate with experience and achievements. Submit *curriculum vitae* and working papers or a defended dissertation proposal, if at the dissertation stage, to: Professor Al Leitch, Moore School of Business, University of South Carolina, Columbia, SC 29208; or Email; leitch@moore.sc.edu. The University of South Carolina is an AA/EEO employer. Minorities and women are strongly encouraged to apply.

627 UNIVERSITY OF HOUSTON Department of Accountancy and Taxation at the C.T. Bauer College of Business invites applications for three tenured or tenure-track appointments at all ranks for Fall 2009. Applicants for senior positions should have a national reputation for scholarship, and a record of excellence in teaching at the undergraduate and graduate levels. Applicants for junior positions must have a doctoral degree prior to the appointment date and exhibit strong potential for and commitment to excellence in research and teaching. We are seeking applications in all areas of Accounting. The C.T. Bauer College of Business and the department are AACSB-accredited. Salaries are very competitive. Please send a copy of your resume, research papers, and relevant teaching materials to: Dr. Gerald Lobo, Department Chair, Department of Accountancy and Taxation, University of Houston, C.T. Bauer College of Business, 334 Melcher Hall, Houston, Texas 77204–6021: or Email: gjlobo@uh.edu. The University of Houston is an Equal Opportunity/Affirmative Action Employer. Women, minorities, veterans, and individuals with disabilities are encouraged to apply.

628 BINGHAMTON UNIVERSITY AT SUNY AACSB-accredited School of Management invites applications for an Assistant Professor position in Accounting for Fall 2009. Candidates must have (or expect) a Ph.D. degree by next Fall. High-quality research as evidenced by publication in the top 5 accounting journals is expected. Research is supported with a two course/semester teaching assignment. The School of Management also supports research with subscriptions to WRDS and other standard databases. The university offers bachelor's, master's and Ph.D. programs in accounting. More details are available at: http://som.binghamton.edu/faculty/index.htm. Located three hours from NYC and one hour from Syracuse and Ithaca, Binghamton is a low-cost-of-living area with 200,000 people. Interested candidates should post a *curriculum vita* and other supporting documents to the appropriate position listing at: http://binghamton.interviewexchange.com/. Application materials to be submitted electronically. Binghamton University is an Equal Opportunity/Affirmative Action Employer.

629 THE GEORGE WASHINGTON UNIVERSITY Department of Accountancy School of Business invites applications to fill a tenure-track position as an Assistant Professor, to begin Fall 2009. Basic qualifications: Applicants should have completed all doctoral degree requirements by August 1, 2009. ABDs will be considered at the rank of instructor and, if appointed, must complete the doctoral degree by the end of the first academic year of appointment. The successful applicant will have a strong commitment and capability for scholarly research and teaching excellence. Salary and benefits are competitive. Application procedure: To be considered applicants should send an application letter, resume, and current working papers to: Professor Keith E. Smith, Chair, Department of Accountancy, School of Business, The George Washington University, Washington, D.C. 20052 (email: accyjob@gwu.edu). Only complete applications will be considered. Review of applications will begin on October 20, 2008 and will continue until the positions are filled. The George Washington University is an Equal Opportunity/Affirmative Action Employer.

630 CENTRAL MICHIGAN UNIVERSITY College of Business Administration is accepting applications for a tenure-track Assistant/Associate Professor position in accounting starting in August 2009 or January 2010. Principal responsibilities include teaching undergraduate and graduate courses in accounting. Normal teaching load is nine credit hours per semester. Some courses are taught online. Required: A doctorate from an AACSB-accredited school with a major in Accounting, or ABD and in the final stages of dissertation completion; evidence of teaching effectiveness in accounting courses at the university level; and a strong research agenda. Preferred: A professional certification (CPA, CMA, CIA) and evidence of online course development and delivery. Please mail application letter, resume, transcripts, and three references to: Dr. Philip Kintzele, Chair, School of Accounting, Central Michigan University, 312 Grawn Hall, Mt. Pleasant, MI 48859; Phone (989) 774–3796; Fax (989) 774–3999. For full job announcement, visit: http://www.fps.cmich.edu/jobs. CMU, an AA/EO institution, strongly and actively strives to increase diversity within its community (see http://www.cmich.edu/aaeo/).

631 INDIANA UNIVERSITY Department of Accounting invites applications for a tenure-track faculty position effective Fall 2009. Preference will be given to Assistant Professors with teaching and research interests in Managerial Accounting. Candidates must possess a Ph.D. and a strong commitment to excellence in scholarly research and teaching. Compensation levels are competitive and negotiable. Interested candidates should send a current *vita*, a research paper, and evidence of teaching ability. Three letters of reference will also be required before an offer of employment can be considered. Applications received before January 1, 2009 will be given full consideration. Contact address: Joe Fisher, Chair, 1309 E. 10th Street, Suite 540, Bloomington, IN 47405–1701; Email: jofisher@indiana.edu. Indiana University is an Affirmative Action/Equal Opportunity Employer committed to excellence through diversity. The University actively encourages applications of women, minorities, and persons with disabilities.

632 INDIANA UNIVERSITY Department of Accounting invites applications for a tenure-track faculty position at the Associate/Full Professor rank effective Fall 2009. Preference will be given to those with teaching and research interests in Financial Accounting. Candidates must possess a doctoral degree and have a strong commitment to excellence in scholarly research and teaching. Compensation levels are competitive and negotiable. Interested candidates should send a current *vita*, a research paper suitable for presentation at a workshop, and evidence of teaching ability to the contact person indicated below. Six letters of reference will also be required before an offer of employment can be considered. Applications received before January 1, 2009 will be given full consideration. Contact address: Joe Fisher, Chair, Department of Accounting, 1309 E. 10th Street, Suite 540 Bloomington, IN 47405–1701; Email: jofisher@indiana.edu. Indiana University is an Affirmative Action/Equal Opportunity Employer committed to excellence through diversity. The University actively encourages applications of women, minorities, and persons with disabilities.

633 NORTH CENTRAL COLLEGE invites applications for a full-time, tenure-track appointment at the rank of Assistant Professor to begin September 1, 2009. The department offers majors and minors in accounting, financial and managerial tracks, and a 5-year bachelor/M.B.A. integrative program. The position entails teaching undergraduate and graduate students in day and evening formats. Minimum qualifications include a master's degree in accounting and CPA designation. Professional experience in the field is preferred. All applicants should exhibit a potential to excellence in teaching and a commitment to scholarship. Preference will be given to those in financial accounting. Strong consideration will be given to candidates interested and able to engage undergraduate students in professional activities outside the classroom. North Central College is a comprehensive liberal arts college of 2,700 students, located 30 miles west of Chicago in Naperville, a dynamic city, along the Illinois research and development corridor. For more information, visit: http://www.noctrl.edu. Send a letter of application, *vitae*, teaching philosophy, transcripts, and three letters of recommendation to: Professor Gerald Thalmann, Search Committee Chair, c/o Academic Affairs Office, North Central College, 30 North Brainard Street, Naperville, IL 60540. Questions concerning this position can be sent to: gathalmann@noctrl.edu. Review of applications will begin November 26 and will continue until the position is filled. Applicants who would enrich the diversity of the campus community are strongly encouraged to apply. Equal Opportunity Employer.

634 NORTHERN KENTUCKY UNIVERSITY Department of Accountancy invites applications for one or more tenure-track positions at the Assistant Professor level beginning in August 2009, with primary teaching responsibility in the areas of financial accounting and auditing. Requirements include a Ph.D. in accounting, commitment to teaching excellence, and demonstrated ability to do quality academic research. Professional certification and practical accounting experience preferred. Salary and benefits are AACSB competitive. NKU is located in the beautiful rolling hills of northern Kentucky just five miles from downtown Cincinnati. The College of Business is AACSB-accredited and offers both undergraduate and graduate degrees including a Master's of Accountancy. Additional information is available at: http://www.nku.edu/~cob. Qualified candidates should submit a letter of application, current resumé, and contact information for three references to: Dr. Carol Lawrence, Chair, Department of Accounting, at: lawrencec5@nku.edu. Northern Kentucky University is an Affirmative Action/Equal Opportunity Employer.

635 THE UNIVERSITY OF TEXAS AT DALLAS (UTD) invites applications for a faculty position in Accounting, effective September 2009. Appointment at Assistant, Associate, or Full Professor level will be considered. Appointees must have (or expect) a Ph.D. or equivalent and demonstrate the ability to conduct a nationally recognized research program. The School of Management at UTD is a research-centered school, ranked 20th in the UTD database that tracks publications in leading business journals (http://som.utdallas.edu/top100Ranking/). Applicants should send a complete *curriculum vitae* and names and addresses of three references to: Academic Search #0141, The University of Texas at Dallas, 800 W. Campbell Road, Mail Station AD 42, Richardson, TX 75080–3021. UTD is an AA/EOE. For more information, visit http://som.utdallas.edu/ or contact Erin Kearney (erin.kearney@utdallas.edu).

637 UNIVERSITY OF MINNESOTA Department of Accounting Carlson School of Management invites applications for a tenure-track position in accounting; rank depending upon qualifications and experience, and consistent with Carlson School and University policy. All applicants must possess a doctoral degree in accounting or a related field (or be close to completion), and must have a strong commitment to accounting research and teaching. Applicants for an Associate or Full position must have demonstrated excellence in teaching and an established record of publications in top-tier academic journals commensurate with the rank they are seeking. Review of applications begins immediately and will continue until the position is filled. Applications should include a *curriculum vita*, samples of recent research papers, evidence of teaching quality, and two letters of reference. In addition, applicants will be required to apply online at: https://employment.umn.edu, and search for posting #58288 (for Assistant Professor) or posting #58287 (for tenured Associate or Full Professor). Applications should be sent to: Recruiting Chair Department of Accounting, 3-122 Carlson School of Management, 321 19th Avenue South, Minneapolis, MN 55455. The University of Minnesota is an Equal Opportunity Educator and Employer.

638 UNIVERSITY OF CALIFORNIA, LOS ANGELES Anderson School of Management invites applications for positions available July 2009 in Financial, Managerial, and Tax Accounting at all academic ranks. A Ph.D. or expected completion by June 30, 2010 required. Demonstrated ability or potential for high-quality research and teaching are important. Applications at the untenured level should include a cover letter, *curriculum vitae*, letters of recommendation, recent publications, or working papers and a job market paper. Applications must be mailed to the contact person by January 15, 2009. Contact: Professor David Aboody, Accounting Department, Rm. D412, UCLA Anderson School of Management, Box 951481, Los Angeles, CA 90095–1481. Under Federal law, the University of California may employ only individuals who are legally authorized to work in the United States as established by providing documents specified in the Immigration Reform and Control Act of 1986. The University of California is an Equal Opportunity/Affirmative Action employer. The department has a strong commitment to the achievement of excellence and diversity among its faculty and staff.

639 BERRY COLLEGE AACSB-accredited Campbell School of Business seeks to fill a tenure-track faculty position in Accounting beginning August 2009. Candidates should have an earned doctorate in Accounting, be excellent teachers, and have an established research agenda with refereed publications. Teaching responsibilities include undergraduate auditing and introductory accounting. Service responsibilities include undergraduate advising. Rank and salary will be commensurate with credentials and experience. Berry College is a comprehensive liberal arts college in northwest Georgia with enrollment of approximately 1,900 undergraduates. Applicants should send a letter of application, current *vita*, evidence of current teaching effectiveness and scholarship, and three letters of recommendation to: Accounting Search, Office of the Dean, Campbell School of Business, Berry College, PO Box 495024, Mount Berry, GA 30149–5024. Screening begins October 1, 2008, and will continue until the position is filled. Berry College is an Equal Opportunity Employer. For ADA Accommodations please contact our ADA Coordinator VTTY 706–236–1722.

641 THE PENNSYLVANIA STATE UNIVERSITY HARRISBURG AACSB-accredited School of Business Administration invites applications for a tenure-track accounting position at Assistant or Associate rank beginning Fall semester 2009. Doctorate in Accounting from AACSB-accredited school required (ABDs who expect to earn their doctorate by August 2009 will be considered). With active research, expect to teach 3/3 following 3/2 in first two years. Teach graduate/undergraduate courses in financial accounting; secondary area in auditing desired. Primary responsibilities are in Harrisburg, may teach at the Lancaster M.B.A. Center. Strong commitment to effective teaching and publication is required. Professional certification (CPA, CMA) and prior teaching, research, professional experience preferred. Compensation is AACSB competitive. For additional details, please visit our website at: http://www.hbg.psu.edu. Review of applications to begin immediately and continue until position is filled. Send *curriculum vita* and four references: Accounting Search Committee, c/o Mrs. Dorothy Guy, Penn State Harrisburg, 777 W. Harrisburg Pike, Box AACSB-28436, Middletown, PA 17057–4898. Penn State is committed to Affirmative Action/Equal Opportunity, and the diversity of its workforce.

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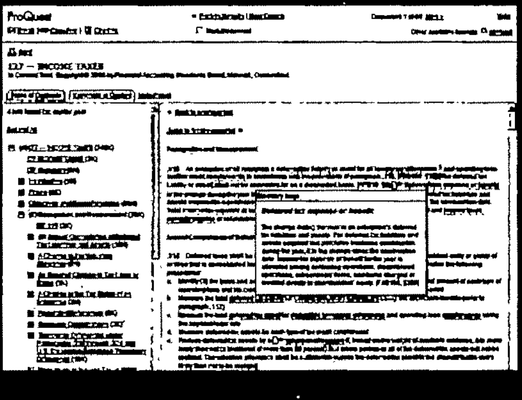

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PEPPERDINE UNIVERSITY
Graziadio School of Business and Management

Seeking Experienced Assistant or Associate Professor in Accounting

The Graziadio School of Business & Management, Pepperdine University, seeks an experienced assistant or associate professor in Accounting starting Fall 2009. We value teaching excellence, a productive research record, and professional experience.

Candidates must have an earned doctorate in Accounting or be in the final stages of their dissertation. A CPA or CMA is desirable. Research support includes access to WRDS (Wharton Research Data Services) with Compustat and CRSP. The school encourages its faculty to engage in consulting activity.

As an AACSB accredited school, we have a full-time MBA program based on our Malibu campus and a large part-time MBA program. This part-time program is taught in the evenings at five locations throughout the Los Angeles area. The teaching needs are primarily in Financial Accounting and Managerial Accounting. Research interests can lie in any of the traditional areas of Accounting.

Applications should include a cover letter, resume, teaching evaluations, a sample research paper, and unofficial transcripts showing degrees earned. For full consideration, please apply by October 1, 2008. Contact: Dr. Richard Powell, Graziadio School of Business, Pepperdine University, 24255 Pacific Coast Highway, Malibu, CA 90263,(310) 506-4874, rpowell@pepperdine.edu.

Dr. Powell will be interviewing candidates at the Annual Meeting of the American Accounting Association to be held in August 2008 in Anaheim.

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UMASS AMHERST

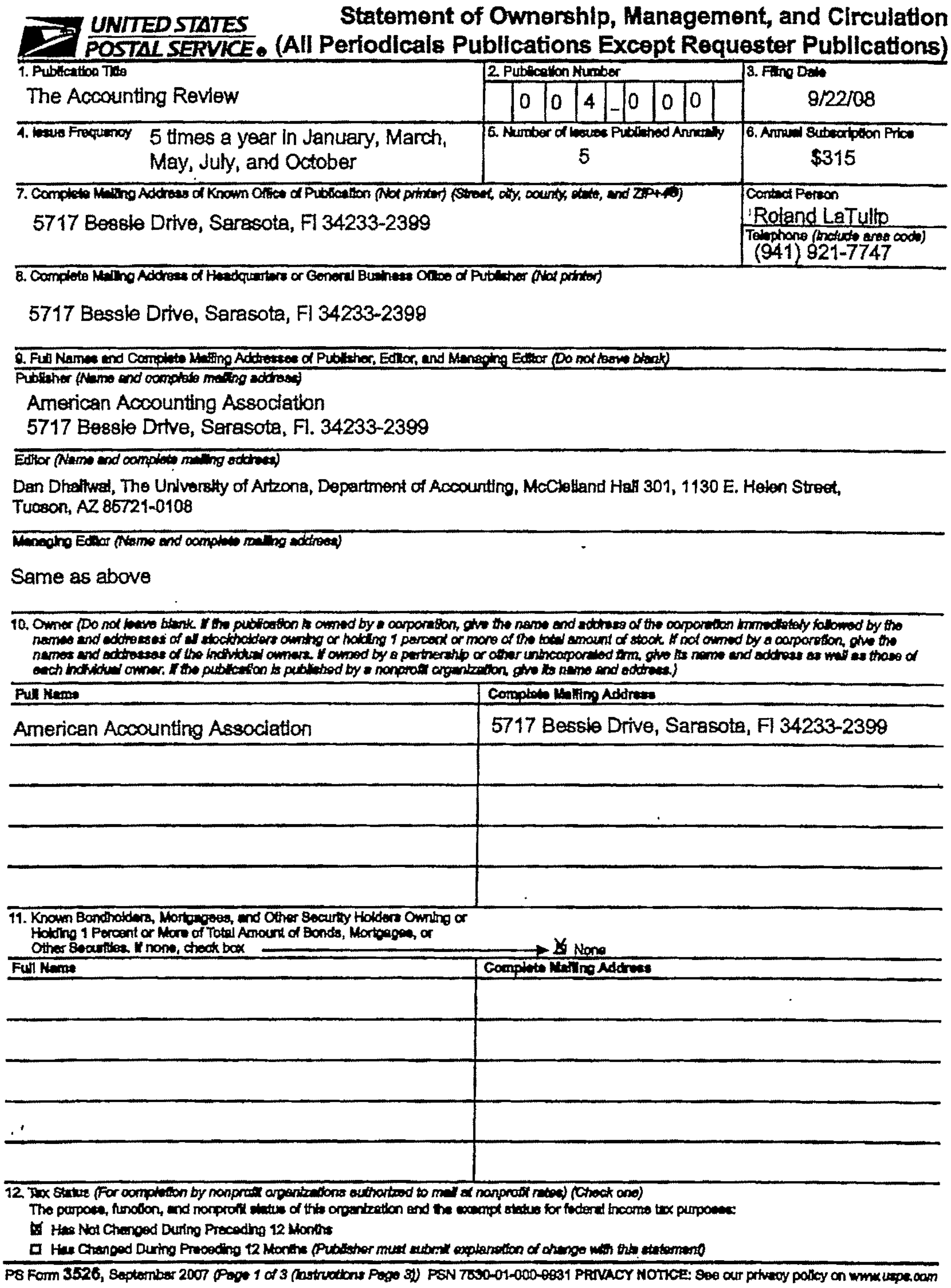
Assistant/Associate Professor Positions

Accounting & Information Systems Department
Isenberg School of Management

The Department of Accounting invites applications for two tenure-track positions as Assistant/Associate Professor of Accounting. All research areas will be considered. Start date is September, 2009. All candidates should have a doctoral degree and a strong commitment to scholarly research and excellence in teaching. The department has a continuing commitment to enhance the diversity of its faculty, student body and curriculum. Salary is competitive. Rank and salary are commensurate with experience and qualifications.

Please send c.v., contact information for three references, and copies of research papers or publications to: Professor James F. Smith, Chair, Search Committee, Department of Accounting & Information Systems, Isenberg School of Management, University of Massachusetts Amherst, 121 Presidents Drive, Amherst, MA 01003. Review of applications will begin on October 31, 2008, but applications will be reviewed until successful candidates are found.

The University of Massachusetts Amherst is an Affirmative Action/Equal Opportunity Employer. Applications from women and minority candidates are especially encouraged.



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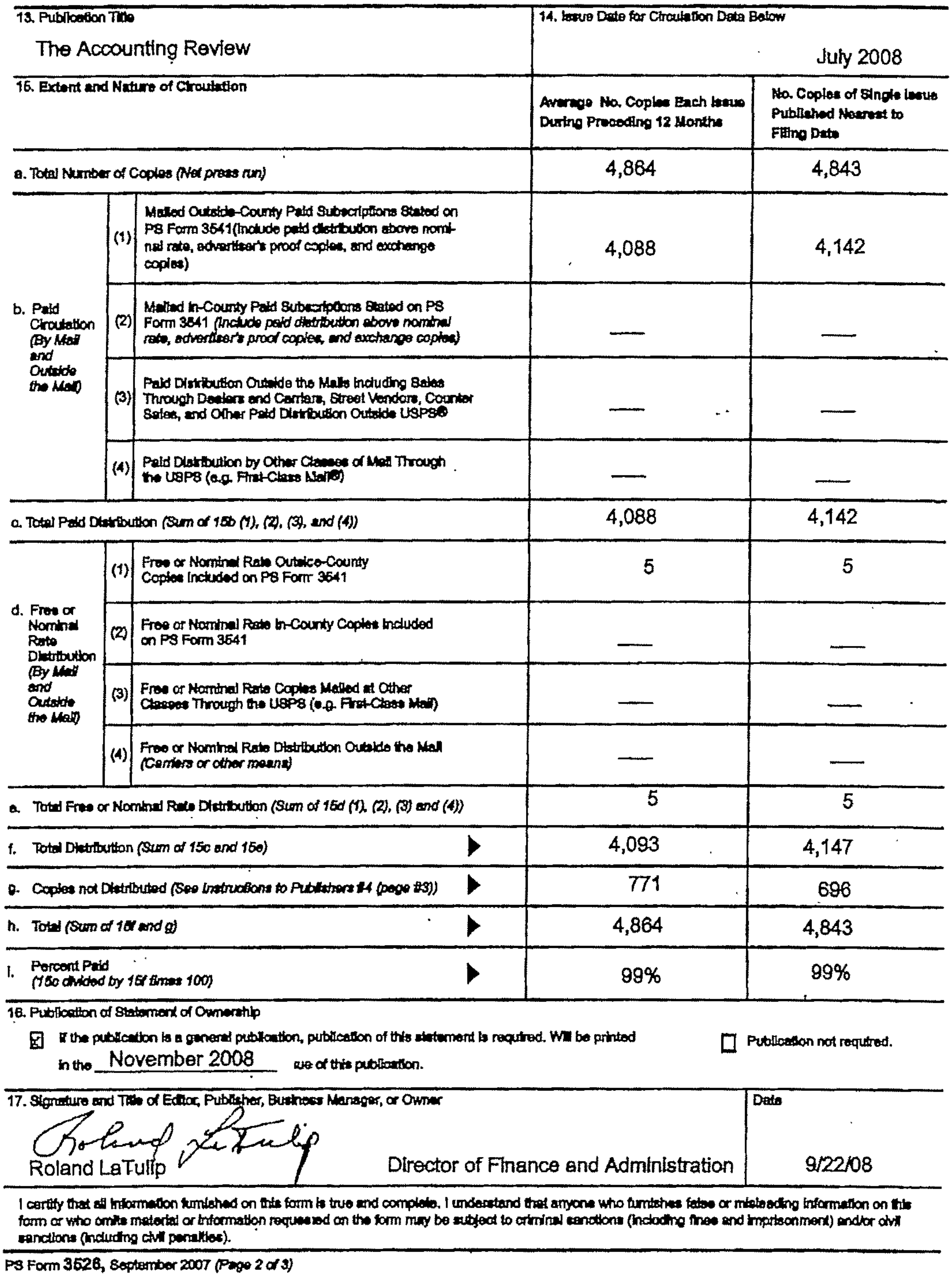

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15. Extent and Nature of Circulation			Average No. Copies Each Issue During Preceding 12 Months	No. Copies of Single Issue Published Nearest to Filing Date
a. Total Number of Copies *(Net press run)*			4,864	4,843
b. Paid Circulation *(By Mail and Outside the Mail)*	(1)	Mailed Outside-County Paid Subscriptions Stated on PS Form 3541(Include paid distribution above nominal rate, advertiser's proof copies, and exchange copies)	4,088	4,142
	(2)	Mailed In-County Paid Subscriptions Stated on PS Form 3541 *(Include paid distribution above nominal rate, advertiser's proof copies, and exchange copies)*	—	—
	(3)	Paid Distribution Outside the Mails Including Sales Through Dealers and Carriers, Street Vendors, Counter Sales, and Other Paid Distribution Outside USPS®	—	—
	(4)	Paid Distribution by Other Classes of Mail Through the USPS (e.g. First-Class Mail®)	—	—
c. Total Paid Distribution *(Sum of 15b (1), (2), (3), and (4))*			4,088	4,142
d. Free or Nominal Rate Distribution *(By Mail and Outside the Mail)*	(1)	Free or Nominal Rate Outside-County Copies Included on PS Form 3541	5	5
	(2)	Free or Nominal Rate In-County Copies Included on PS Form 3541	—	—
	(3)	Free or Nominal Rate Copies Mailed at Other Classes Through the USPS (e.g. First-Class Mail)	—	—
	(4)	Free or Nominal Rate Distribution Outside the Mail *(Carriers or other means)*	—	—
e. Total Free or Nominal Rate Distribution *(Sum of 15d (1), (2), (3) and (4))*			5	5
f. Total Distribution *(Sum of 15c and 15e)* ▶			4,093	4,147
g. Copies not Distributed *(See Instructions to Publishers #4 (page #3))* ▶			771	696
h. Total *(Sum of 15f and g)* ▶			4,864	4,843
i. Percent Paid *(15c divided by 15f times 100)* ▶			99%	99%

16. Publication of Statement of Ownership

☒ If the publication is a general publication, publication of this statement is required. Will be printed in the November 2008 issue of this publication. ☐ Publication not required.

17. Signature and Title of Editor, Publisher, Business Manager, or Owner Roland LaTulip Director of Finance and Administration	Date 9/22/08

I certify that all information furnished on this form is true and complete. I understand that anyone who furnishes false or misleading information on this form or who omits material or information requested on the form may be subject to criminal sanctions (including fines and imprisonment) and/or civil sanctions (including civil penalties).

PS Form **3526**, September 2007 *(Page 2 of 3)*

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